

# Antiquity

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## Editorial Notes

DATING the past in years, and not merely in periods, is the aim of archaeology. The C 14 method just discovered will probably be used to date all sites in future, but its range extends only to about 25,000 B.C., and man existed long before that. Now comes a new technique which promises to carry the story back beyond the limits of C 14. It is based upon the fact that the deep ocean deposits still forming have been forming continuously over a very long period of time. They are of course stratified, and by making a boring through them and analyzing the contents of the core, certain very important conclusions can be drawn about the climatic changes that took place during and after the Ice Age. These agree quite well with what was already known, and will enable us to give dates in years—i.e. an absolute chronology—to the phases of the Old Stone Age. We hope shortly to publish an article on this subject. Meanwhile we quote (with the permission of the Editor and Author) from an article which appeared in *The Times*, 22 Sept. 1950, p. 5. It is by Dr J. D. H. Wiseman, of the British Museum (Natural History). 'As the distance from the top of the core of the sample corresponding with the climatic optimum is known, it is possible to calculate with a considerable degree of accuracy the age of any portion of the cores, provided the distribution of calcium carbonate as well as the original water content is known. It is in this way possible to assign dates to climatic oscillations. About 117,000 B.C. there was a distinct but sharp minimum temperature. Thereafter the temperature rapidly rose, and with some minor oscillations reached a maximum at 78,000 B.C. The temperature then fell to a minimum at 68,000 B.C. which was not as cold as the minimum at 117,000 B.C. After that the temperature increased until 43,000 B.C.; it then remained essentially constant for 16,000 years. There was a distinct fall in temperature and at 23,000 B.C. there was a minimum which was not as cold as the two preceding minima. The temperature then increased, remained for some time essentially constant, and at 10,000 B.C. there was an indistinct minimum. After that the temperature rose rapidly until 7,000 B.C., when there was a more gradual rise until the climatic optimum at 3,000 B.C. was reached. These dates are approximate, as further investigations might lead to minor modifications'.



Every year the British Association for the Advancement of Science publishes a brochure containing the presidential addresses delivered to its thirteen sections. They provide an admirable conspectus of the general state of knowledge in the field of science, and though unfortunately there are always some presidents who lack the powers of lucid expression, they make good reading on the whole. The address delivered to Section L

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(Education) by Mr W. F. Oakeshott, headmaster of Winchester College, is short but stimulating. If we have understood him correctly, he laments the cleavage which began (as he thinks) soon after the middle of the 19th century, when a general acquaintance with the state of scientific knowledge gradually ceased to be part of the normal educated person's mental equipment. 'The educated man of the 18th century still regarded the discoveries of the scientist as being part of his normal reading'; and Darwin's *Origin of Species* and Lyell's *Principles of Geology* were 'widely read by men who were not primarily scientists'. The reason alleged for this cleavage is that the specialist no longer feels an obligation to write books for the layman because that service is now performed by others. Archaeology is cited as an example. 'What is wanted is that the expert archaeologist . . . should write books for the general public'. We rubbed our eyes in amazement when we came to this sentence. Has Mr Oakeshott, who calls himself an 'interested amateur' of archaeology, never heard of the many books written during the last two or three decades by 'expert archaeologists', some of them by a distinguished Wykehamist? Is there any 'expert archaeologist' in this country, indeed, who has not written at least one such book? Has he never heard of the Penguins and Pelicans, and that some of them are prescribed as standard advanced text-books in American universities? Has he never heard of ANTIQUITY? Nor does his accusation hold against specialists in other branches of science; there have been many such books published. The fault lies not with the experts but with educators who fail to use their books or even, as it seems, to know that such exist.



What is the result? The higher posts in the Civil Service are filled with people who are ignorant of the most vital knowledge of our age, and consequently out of sympathy with that spirit of research which animates and is the driving force of many lives. We may be too optimistic, but we cannot believe that bureaucrats are all as bad as Mr Oakeshott's former chief who (he says) enjoyed 'the power [his post] gave him of doing harm to better men than he was'. We have all met people of this kind, but we cling desperately to the theory that ignorance rather than mere love of power is responsible. In fact, like Mr Oakeshott, we prefer St. Benedict's view of human nature to that of Machiavelli. Ignorance not malice is surely responsible for most of the misunderstandings that arise between the world of learning and science on the one hand and that of politicians and administrators on the other. For that ignorance our educational system must bear part of the blame; but the remedy is not easily found, for that system is controlled in the last resort by its own products—or victims.



# The Zürich Congress\*

by STUART PIGGOTT

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WHEN the Editor of *ANTIQUITY* invited me to write some notes on the Zürich Congress, it was agreed that they could only be considered as personal impressions, and not a formal report. The unfortunate human limitation which debars one from being in two places at once is always irritatingly obtrusive on such occasions, and a decision on which of several simultaneous papers to attend is likely to be decided by spinning a coin at the last moment, so that one can do no more than record one's own impressions, and something of those of friends to whom one has talked.

In general, I think we all carried away a feeling that the Congress had been a great success, with an admirable blend of hard work and informal enjoyment; the arrangements made by our Swiss hosts were altogether excellent in their unobtrusive efficiency. About 250 individuals had enrolled as members of the Congress, and most were present, and their proportion by countries reflects the post-war state of European archaeology—37 from the Scandinavian countries, 26 from Germany, and nine from Austria; 24 English members, with six from Eire; Holland with 17, France with 16 and Italy with 14 members. Spain and Belgium were each represented by five members, and the United States by eight, and the remaining countries represented by two or more delegates. Mr G. M. Young once defined a university in a sentence which began 'A University is a place where young men and women educate one another by conversation,' and this phrase may equally well apply to a Congress—it is the informal conversations over a drink that are often the most valuable part, and at Zürich there was plenty of such discussion, in which language barriers were always somehow overcome even by the most embarrassed monoglots.

When one comes to consider the interests of the members as indicated by the papers read, one fact immediately stands out. Palaeolithic studies, which with the addition of a few Mesolithic contributions, were represented by 25 papers, are becoming, or have become, a closed shop for most of the European prehistorians whose concern lies in this field. This is the French tradition, perpetuated indeed in the title of the Congress, whereby a *préhistorien*, concerned with the palaeolithic and (grudgingly) with the mesolithic periods, is sharply differentiated from the student of *protohistoire*, whose province stretches from the neolithic to the Dark Ages; in one otherwise excellently arranged large provincial French museum I found this summer that the neolithic material (stones, therefore *préhistoire*) was in a room remote from the protohistoric collections covering the Bronze Age to the Migration Period. At Zürich, protohistorians could be seen briskly moving between the 3rd millennium B.C. and the 6th century A.D., but the prehistorians almost to a man remained in their palaeolithic seclusion. In England this artificial distinction has never been formed, and one of the most outstanding papers given to the *préhistoriens* was the report on the Seamer mesolithic site by Dr Grahame Clark.

The main weight of papers however, exclusive of the palaeolithic series, lay in the European Bronze Age, with 22 papers as against 11 dealing with the Neolithic, 12 with

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\* The Third International Congress of Prehistoric and Protohistoric Sciences, Zürich, 14-19 August, 1950.

the Early Iron Age, and 15 with the Dark Ages. In the Neolithic section, Professor O'Riordain gave a stimulating paper based on his recent Lough Gur excavations, Dr Mathiassen presented the new views current on the beginnings of the Danish neolithic cultures, and Dr von Gonzenbach summarized her recently published classification of the Cortaillod culture of Switzerland (1). Professor von Merhart's paper on Late Bronze Age chronology was a brilliant *tour de force*, turning largely on the controversial questions of the bronze vessels discussed in the previous communication by Professor Childe: the Late Bronze Age was again brought under review in papers by Professors Sprockhoff, Hawkes, and Kimmig and by Mr Cowen. Questions of Early and Middle Bronze Age chronology were dealt with in papers by Professor Schaeffer, M. Giot, and the writer. Schaeffer reiterated his case for a high dating of the Central European Early Bronze Age, based on Oriental types of pin and 'ingot-torque'; the chronology of the 'Wessex Culture' was reviewed in the light of recent work (particularly that of de Navarro), (2) and M. Giot presented the first up-to-date account of the Breton Bronze Age since du Chatellier's day.

In the Early Iron Age section, the most controversial communication was that of Dr Raftery, claiming a La Tène date for Cairn H at Loughcrew (and by inference other chambered cairns of the same type) in the light of his excavations there: judgment on this bold claim must obviously be reserved until the publication of the full report. In the Dark Ages, important papers were read by Professor Werner, and Mr Bruce-Mitford displayed the wonders of Sutton Hoo.

Professor Vogt's public lecture on the problems of the Swiss Neolithic and Bronze Age was, as might be expected, a notable contribution, but one paper of great general interest needs special mention—that of Professor Braidwood on Carbon 14 dating in relation to Oriental prehistory. He began by describing in outline the theory of radioactive carbon dating, now familiar to readers of *ANTIQUITY*, and then gave the results of recent examination of organic materials from certain Near Eastern sites. His own recent excavations at Jarmo (3), in the Iraq hills, have brought to light a settlement of people who were agriculturalists, growing wheat and barley and domesticating animals. Though clay-lined hearths were present, pottery appears to be unknown, though there was an industry in stone, chipped, flaked or polished, and unbaked clay figurines. Such a culture should, on general grounds, be early on the Mesopotamian sequence, and possibly antedate the beginnings of Hassuna, so that the Carbon 14 dates might be expected to be high. The figures were in fact  $6890 \pm 320$  years Before Present, giving a corrected date of c. 5260–4620 B.C.—a by no means fabulous antiquity. In this connection further C 14 dates are interesting—Fayum A, with dates of  $6219/6304 \pm 330$  B.P., or c. 4784–3929 B.C.; El Omoni, c. 3680–3220 B.C., and Alishar Chalcolithic, c. 2950–2450 B.C. Material from Hemaka, dated archaeologically to the middle of the First Dynasty, gave a date of c. 3397–2734 B.C. Professor Braidwood stressed the fact that these dates are still regarded by the physicists as highly provisional, but at all events they are a beginning. One hopes that other determinations will be made available before long, and especially, in the Near Eastern context, that there may be Natufian material that could be used for C 14 examination.

<sup>1</sup> *Die Cortaillodkultur in der Schweiz* (Basel, 1950).

<sup>2</sup> 'The British Isles and the Beginning of the Northern Early Bronze Age,' in *Early Cultures of N.W. Europe* (Chadwick Essays, 1950).

<sup>3</sup> See his article in this number, pp. 189–95.



## THE ZÜRICH CONGRESS

Apart from the papers read at the Congress, attention should be drawn to the excellent film, made by M. France-Lanord in the technical laboratory of the museum of Nancy, demonstrating the investigation of pattern-welding in Dark Ages sword-blades. This was exhibited by M. Salin, and raised the hopes of many of us that further investigations should be carried out in this country, not only on Dark Ages weapons (4), but on Early Iron Age swords as well. Professor Vogt had also arranged a special exhibition of drawings to illustrate current Swiss technique in the recording of plans and sections in archaeological excavations, and their subsequent preparation for publication. As might be expected, the standard was very high, the working drawings being usually executed in coloured pencils and, in the case of the sections in particular, being drawn in great detail in an attempt to achieve fidelity in copying each variation of tone and colour in the soil. The whole question of representation and convention in archaeological section-drawing is one which deserves full-length discussion, but the direct half-tone reproductions of some of the Swiss drawings seemed to be very unsatisfactory as illustrations to the excavation reports, and one felt that some of the records—for instance a plan of a group of post-holes in which every natural crack and crevice in an expanse of rock surface many square metres in area had been laboriously drawn—were really unnecessarily elaborate and added nothing to the value of the diagram. After all, any archaeological draughtsmanship is a matter of working to a set of black-and-white conventions, and an attempt at producing an allegedly completely objective and accurate record of the appearance of the soil is bound to fail. Here, as in all aspects of an excavation, the excavator's responsibility does not end with pure record, but must always include the interpretation which he alone, working day after day in the closest contact with the site, can properly apply to its problems.

A mid-week excursion was made from Zürich to visit the palaeolithic rock-shelters of Schweizerbild and Kesslerloch, dug in the last century, and also to a most interesting new excavation, made specially for the Congress, on the well-known 'lake-dwelling' site at Weiher, of the Michelsberg culture. The area exposed included vertical piles and a section of a corduroy road, while the presence of a tree-stump *in situ* implied, as Dr Bersu pointed out on the spot, that the settlement had not been raised above the water, but was actually founded on more or less dry land—the site in fact must have been on an island in a lake formerly filling the little valley, now dry and cultivated. A similar demonstration excavation was also visited at the Ile Werd near Stein-am-Rhein, where a good stratification of neolithic and Schnurkeramik levels was exposed. After the formal Congress week, the new excavations at the classic site of Auvernier, on Lake Neuchâtel, were visited, where M. Perret is testing the stratification established by Vouga, and uncovering a considerable area of the earlier Cortaillod level—a very fine wooden knife-handle is one of the most striking finds from this layer. The results of this excavation, taken in conjunction with those of M. Leroi-Gourhan's test dig at the same site (5), should do much to clarify our knowledge of the culture-sequence at Neuchâtel.

In addition to the official programme, the Congress published a very valuable *Guide d'Excursion*, with notes, bibliographies and illustrations of the sites visited after the Congress, and the Schweizerischen Gesellschaft für Urgeschichte dedicated a special volume of its *Jahrbuch* (Vol. XI, for 1949-50) to the Congress, containing a most valuable series of papers on the archaeology of Europe and beyond: Professor Childe's study of the contrasted areas of use of axe and adze, bow and sling, in prehistory, dealing with the

<sup>4</sup> Cf. Mr Herbert Maryon's work in *Camb. Ant. Soc. Proc.*, xli, 73.

<sup>5</sup> Preliminary report in *Ur-Schweiz* XIII (1949), 1-6.

Orient as well as areas nearer home. Incidentally, his remarks on the use of slings in the Near East receive additional confirmation from Professor Wheeler's recent discovery of sling-stone hoards on the Citadel at Mohenjo-daro.

Finally, it was both interesting and gratifying to perceive a growing realization among the scholars of Continental Europe that an increasing number of British prehistorians were concerned, not only with the problems of their own islands, but with those of Europe as a whole. French prehistory (or strictly, *protohistoire* !) is in particular yielding to a pincer movement from Britain and Germany, both in the neolithic and in the later Bronze Age, and the Mediterranean is a common area of study for Spain, Italy and Britain. After the personal contacts established during the Congress, such work will proceed with increased ease, and by constant co-operation between colleagues scattered throughout Western Europe some main outlines in prehistory at least should be established.



# The Antiquity of *t'ou hu*

by RICHARD C. RUDOLPH

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SOME ten years ago Professor G. Montell published a well-illustrated and highly interesting account of the history of the Chinese game of *t'ou hu*<sup>a</sup> or Pitch-pot (1). In this game, practised in China for many centuries, two contestants attempt to toss (*t'ou*) arrows into the mouth of a narrow necked vessel (*hu*) according to very elaborate rules of procedure. He began by quoting verbatim Legge's translation of the lengthy description of the game as contained in the *Li chi*,<sup>b</sup> (2) taking this as evidence that the game 'was perfected as early as the Chou period (3)'. He surmised that the game survived the centuries although he knew of 'no further mention of it in the literature until the time of the Sung dynasty (4)', his next source dating from the 11th century (5).

Two 2nd century illustrations of the game, and various literary references prior to the Sung dynasty (960-1280) having come to my attention, I offer this additional material as a supplement to Professor Montell's illuminating article.

The game of pitch-pot undoubtedly existed as early as the Chou dynasty, but the text of the *Li chi* that has been handed down can hardly be considered as the most reliable evidence for this. It is now well known that although the *Li chi* purports to describe ceremony as it existed during the latter Chou period (1122-255 B.C.), it actually was written in the Early Han (206-25 B.C.) dynasty. It is, indeed, better evidence than none for the Chou period, and one should remember that earlier documents were undoubtedly used in its compilation. What is perhaps more reliable evidence for the existence of this game during the Chou period can be found in the *Tso chuan*.<sup>c</sup> There, under the twelfth year of Duke Chao which corresponds to 530 B.C., we are told that a noble on a mission to the State of Chin indulged in the game of *t'ou hu* or pitch-pot. It is possible that here again the writing may not be contemporary with the alleged event, but nevertheless most scholars who have investigated the authenticity of the *Tso chuan* agree that it was written in the latter years of the Chou dynasty. Moreover, it is generally believed that the compiler had access to the court archives of the various feudal states and it is probable that in this case a contemporary record was used. It hardly seems likely that the compiler of a historical account would go so far as to interpolate such a passage. The text is as follows:—

<sup>1</sup> 'T'ou Hu—The Ancient Chinese Pitch-pot Game', *Ethnos*, v (1940), 70-83.

<sup>2</sup> *The Sacred Books of the East*, xxviii (Oxford, 1885), 397-401. Cf. S. Couvreur, *Li Ki ou mémoires sur les bienséances et les cérémonies*, II (Ho Kien Fou, 1913), 591-9. A parallel text from the related *Ta-tai li-chi*,<sup>c</sup> ch. 78, has been translated by R. Wilhelm in his *Das Buch der Sitte des älteren und jüngeren Dai* (Jena, 1930, 333-6. Wilhelm comments that this section is based upon earlier sources.

<sup>3</sup> Montell, op. cit., p. 74.

<sup>4</sup> Ibid.

<sup>5</sup> This is the *T'ou-hu hsin-ko* by Ssu-ma Kuang.<sup>d</sup> Cf. the *T'ou-hu i-chieh* by Wang T'i,<sup>e</sup> a 16th century work on the game.

' While the prince of Chin, attended by Chung-hang Mu-tzu, was entertaining the prince of Ch'i, pitch-pot was played. The prince of Chin was first and Mu-tzu said to him, " You have liquor equal to (the volume of) the Huai River, you have meat equal to a hillock. If my master succeed in this, he shall be leader of the princes ". He did succeed and the prince of Ch'i raised an arrow and said, " I have liquor equal to (the volume of) the Sheng River, I have flesh equal to a mound. If I succeed in this, I shall rise up and take your place ". He also hit the mark and Po-hsia said to Mu-tzu, " You erred in your speech. We are certainly the leader of the princes. That pot—why did you use it? How should a successful throw into it give any superiority? . . . " (6) '.

Next in chronological order comes a reference to pitch-pot in the *Shih chi*.<sup>6</sup> This remarkable historical work was compiled in the 1st century B.C. from reliable court archives. Although some embroidering was done by later writers, this is mainly limited to fictional conversations and flattering descriptions of events or persons. The *Shih chi* reference to the game occurs in chapter 126, that devoted to biographies of famous wits. Pitch-pot is mentioned among the pastimes of party-loving Shun-yü k'un<sup>h</sup> who is supposed to have lived in the early part of the 4th century B.C. Whether this is legitimate 4th century evidence of pitch-pot is debatable; but its authenticity as a first century B.C. reference to the game cannot be denied.

Another reliable early source that mentions the game is the *Hou-han shu*,<sup>7</sup> the *History of the Late Han Dynasty*. Although put in its final form in the 5th century, it is undoubtedly compiled from contemporary Han records. In the biography of Chai Tsun,<sup>8</sup> (7) a military leader and idealist who flourished during the 1st century, we are told that he laid great emphasis upon the game of *t'ou hu*. Li Hsien,<sup>k</sup> the 7th century commentator on this history, refers to the *Li chi* for information on *t'ou hu*. This suggests that no other works on the game were extant in his time.

The above sources are only a few of some twenty pre-Sung literary references to *t'ou hu* that I have seen in various texts and reference works. Let this suffice for the texts while we turn to actual pictures of the game as it was played in the 2nd century.

Unquestionable pictorial representations of the game are depicted in two stone reliefs dating, at the latest, from the 2nd century A.D. Both of these reliefs were decorations on Late Han stone tombs discovered some years ago around Nan-yang in Honan province (8). Figure 1 shows a clear and unmistakable representation of two kneeling men playing at the game of pitch-pot. Each one holds arrows over one shoulder and tosses an arrow at the vase which is between them. One should expect both men to be holding their supply of arrows with their left hand and tossing with their right, but in this relief the artist, perhaps for the sake of symmetry, shows one man holding his arrows in the above manner, and the other holding his arrows with his right hand while he tosses with the left. Each man seems to have been supplied with five arrows; there are two in

<sup>6</sup> Cf. James Legge, *The Chinese Classics*, vol. v, *The Ch'ün Ts'eu with the Tso chuen* (Hong Kong, 1872), 639. My wording differs.

<sup>7</sup> *Hou-han shu*, ch. 150.

<sup>8</sup> *Nan-yang han hua-hsiang hui-ts'un* (Nanking, 1937), 66b, 67a. An earlier work on the Nan-yang reliefs, *Nan-yang han hua-hsiang chi*<sup>m</sup> (Shanghai, 1930), does not contain these scenes.

Although the rich Han reliefs from Shantung published by Chavannes and others, and the virtually unknown Han reliefs from Szechwan (soon to be published by this writer) contain numerous scenes of pastimes and entertainment, none show this game. To my knowledge, the Nan-yang reliefs provide the only illustrations in Chinese art of this game prior to the 11th century.



the vase and each contestant still has four in his possession. According to the *Li chi*, four arrows were used by each person. Beside the vase is a vessel with a ladle. In back of the man on the right is another person who is probably an attendant whose duty it was, according to the account in the *Li chi*, to keep tally. This scene is three-quarters of the picture depicted on the entire slab which measures 128 by 34 centimetres. The portion of the scene not shown here consists of two people who have no relation to the game. Figure 2, one-half of the other Nan-yang tomb relief, is much less distinct but undoubtedly represents the same game. The original was approximately the same size as the other. As in the other figure, to the left of the pot is seen a vessel on a stand.

a	投壺	h	淳于髡
b	禮記	i	後漢書
c	大戴禮記	j	祭遵
d	投壺新格, 司馬光	k	李賢
e	投壺儀節, 汪禔	l	南陽漢畫象彙存
f	左傳	m	南陽漢畫象集
g	史記	n	容庚, 適周彝器通考

### ○ 雙劍謠吉金圖錄

#### KEY TO transliterations in this article

Aside from their over-all value as pictorial evidence of the game in the 1st or 2nd century, the reliefs provide other specific information of interest: Examples of bronze vases for this game seen by the writer in the Royal Ontario Museum (Toronto) differ considerably from those shown in the rubbings of the Nan-yang reliefs. The examples referred to, dating from the 17th or 18th centuries, did bear some resemblance to the vase in Fig. 1 in that they had long, narrow necks and round, squat bodies. But they differed from the vases in both illustrations in that they had vertical tubes or rings attached to either side of the neck. Both the 11th and 16th century works referred to in Note 5 provide the vase or *hu* with these rings at which arrows were also aimed. The detailed account in the *Li chi*, quoted by Montell, does not mention such rings or tubes and in this respect agrees with the Nan-yang reliefs of the 2nd century. If these reliefs illustrate 'standard' equipment for the game, then the tubes, or 'ears' as they are called in the Chinese texts, were added sometime between the 3rd and 11th centuries.

But *hu* with 'ears' were made in China as early as 12th or 13th centuries B.C., although they were eliminated by Han times. Thus the use of 'ears' on the vases

designed for the later versions of the game may have been inspired by very early examples (9),

Jung Keng in his discussion of the *hu* (10), although he quotes the *Li chi*, does not allude to its use in this game. Since *t'ou hu* apparently was conducted in a ritualistic manner, it would not be surprising if bronze *hu* were cast for this specific purpose.

To summarize the new data on the Chinese game of *t'ou hu* that is presented here, we may say that on the whole they substantiate Professor Montell's conclusions, that the literary evidence adds conclusive proof that the game existed in the Chou dynasty and was practised through the following centuries, and that the 2nd century reliefs are in general accord with the description of the game written some four centuries earlier but differ from later accounts.

*Postscriptum* : Some time after I wrote the above article, Professor L. C. Goodrich of Columbia University kindly drew my attention to the illustration of a group of Han dynasty tomb figures shown here as FIG. 3. This was described in the *Illustrated London News* (11) as probably being a representation of the game of mah-jong, an impossibility because this game is of much more recent origin—probably around the 11th century. This group of figures, probably dating from the 1st century, consists of two players on either side of a gaming board with an attendant in the background. On the board are five or six wand- or arrow-like objects and a large vase. The Han dynasty game of *liu po* (12) comes to mind in connection with six such objects and a gaming board, but the presence of the large pot eliminates this possibility and forces one to conclude that this group is intended to show a game of *t'ou hu* in progress thus supplementing the two reliefs by showing in the round the same action that they portray in profile. The positions of the hands of the players indicate that they originally held wooden arrows.

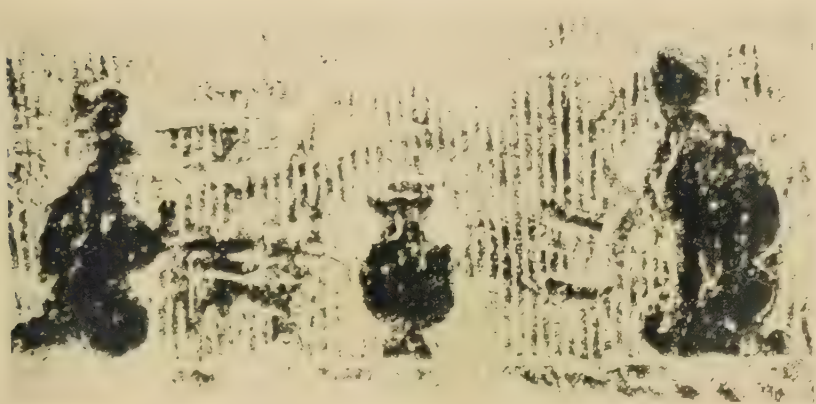
<sup>9</sup> Jung Keng's monumental work on ancient Chinese bronzes shows a number of very early *hu* with vertical tubes on the neck, probably used for slinging the vessel. His illustrations also show the change from tubes to free swinging ring handles prior to the Han dynasty. See Jung Keng, *Shang-chou i-ch'i t'ung-k'ao*<sup>a</sup> (Peiping, 1944), II, nos. 704-83. Bronze vessels of the *hu* type were commonly inscribed but I have looked in vain through collections of inscriptions for reference to the game. Several Chinese works of Han bronzes contain illustrations bearing some resemblance to the two vases shown in our reliefs. One *hu* in particular bears a striking similarity to the one in Fig. 2. See *Shuang-chien-ch'ih chi-chin t'u-lu*,<sup>o</sup> II, 54. See also Charles Fabens Kelley and Ch'en Meng-chia, *Chinese Bronzes from the Buckingham Collection* (Chicago, 1946), pl. LXII, LXXV.

<sup>10</sup> op. cit., I, 432-5.

<sup>11</sup> 13 May 1933, p. 694, fig. 5.

<sup>12</sup> This game seems to have been widely played during the Han period but early Chinese literature provides but meagre information on it. See Liang-sheng Yang, 'A Note on the so-called TLV Mirrors and the Game Liu-po', *Harvard Journal of Asiatic Studies*, 9 (1945-47), 202-206.









# ‘Ape-Men’ of South Africa

by W. E. LE GROS CLARK

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IN 1925, Professor Dart of the Witwatersrand University described the well-known Taungs skull (*Australopithecus*). He pointed out that, in spite of the obviously ape-like proportions of the brain, there were certain features (particularly in the dentition) in which this fossil primate resembled Man more closely than do any of the known anthropoid apes. In general, other anatomists agreed that this certainly was so, but some regarded the resemblances as little more than interesting examples of parallelism having no particular reference to the origin of the human family (Hominidae). In 1936 and the following years, Dr R. Broom, F.R.S., of the Transvaal Museum, discovered many more remains of similar ape-like creatures, including teeth, portions of skulls and jaws, and parts of the limb skeleton. Incidentally, he believed these remains to represent two types closely related to *Australopithecus* but generically distinct, and he gave them the names *Plesianthropus* and *Paranthropus*. Whether this generic distinction will be accepted as valid is a subject of controversy. Actually, however, it is (for the moment) a matter of no great importance, for it is agreed that they all certainly belong to the same group, the Australopithecinae. Dr Broom’s account of his discoveries (which were made at Sterkfontein and Kromdraai—not far from Johannesburg) was published in 1946 (1). It became clear from his description and illustrations that the hominid traits of the Australopithecinae were even more remarkable than many anatomists had supposed. So much was this the case, indeed, that some of those who had shown a cautious reserve towards the Taungs skull now expressed their conviction that these extinct primates were by no means merely ‘anthropoid apes’. On the contrary, they agreed that they provided evidence of the utmost importance for problems of human evolution. But although this now became widely accepted, one or two critics continued to combat it with rather unusual (and also rather puzzling) vehemence. Meanwhile, unperturbed by these minor protests regarding his earlier discoveries, Dr Broom was quietly working away at his excavations at Sterkfontein, and in 1947 he came upon an astonishingly rich collection of Australopithecine remains closely packed together in a small and circumscribed locus of dense limestone. This new material proved to be much more complete and perfectly preserved than anything hitherto found in the area, and included several skulls or parts of skulls, many teeth, and an exceptionally fine specimen of a pelvic bone.

A monograph on the Australopithecine remains discovered at Sterkfontein since the beginning of 1947 has now been published as a memoir of the Transvaal Museum, Pretoria (2). Dr Broom and his assistant, Mr J. T. Robinson, deal with the skulls, jaws, teeth and limb bones, while Dr Schepers contributes an account of the endocranial casts of three of the skulls. The description of the skeletal material is straightforward and comprehensive, but is not intended to be in any way exhaustive. The authors are concerned mainly to give the facts. But in their introduction they permit themselves

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<sup>1</sup> R. Broom and G. W. H. Schepers. ‘The South African Fossil Ape-men’, *The Australopithecinae*, Transvaal Mus. Mem., No. 2, 1946.

<sup>2</sup> R. Broom, J. T. Robinson and G. W. H. Schepers. ‘The Sterkfontein Ape-man’, *Plesianthropus*, Transvaal Mus. Mem., No. 4, 1950.

the mild remark that, in addition to the facts, 'we must be allowed to give what are our own very definite opinions'. This, of course, is very right and proper, though it is perhaps hardly surprising if Dr Broom supposes that some of his earlier critics may have thought otherwise. No doubt the most important specimens of the new Sterkfontein material are the practically complete and undistorted skull (Sterkfontein skull No. 5), and the almost complete pelvic bone. The skull is a truly remarkable find. One of the most impressive illustrations (among many which are noteworthy for their excellence) is the photograph of Dr Broom inspecting it when it was first discovered. This photograph is particularly useful since it serves to emphasize the great density of stalagmitic matrix in which all these fossils were encased. There is no question here of the fossil remains (which, incidentally, show not the slightest evidence of 'rolling') being scattered in a loose sedimentary deposit which might have allowed the chance intrusion of extraneous material to form a heterogeneous mixture of fossils of different geological age. It has only been possible to expose the fossil specimens by blasting the dense rock in which they were sealed up, and it was a happy chance that, on 18 April 1947, a blast charge detached a lump of limestone with the roof of a skull, leaving the remainder intact and still embedded in the parent rock. Dr Broom, with consummate skill, was able to extract the whole skull from its matrix, and found that the calvarial roof fitted quite accurately in position. The importance of this skull lies in its completeness and good preservation. It thus allows a more accurate comparative study of the Australopithecine skull than has hitherto been possible.

But apart from such fine specimens as this, the great quantity of material which Dr Broom has made available by his energetic excavations has this most important result—it now enables us to take a comprehensive view of the total morphological pattern presented by the Australopithecinae, with no further excuse for relying for the assessment of affinities on individual characters of individual skeletal elements treated as isolated abstractions. In fact, the need for a synoptic study of the *whole* morphology of these extinct primates has lately become very obvious, for some of the statistical studies limited to individual metrical abstractions (without any attempt to view the situation as a whole) have evidently led to quite anomalous conclusions.

So far as the skull is concerned, there are certainly a number of individual hominid features (as I have pointed out elsewhere (3)) which do not appear to find an exact parallel in any of the known apes, and others which may occasionally be found *singly* in an ape's skull if diligently sought in a large enough series. However, in a comparative study reliance is not necessarily to be placed on any one of these features taken by itself, but rather on the *total pattern which they present in a particular combination*.

Among the approximately hominid features, which are to be found in the Australopithecine skull, are the relatively low position of the area for the attachment of the neck muscles, the relatively forward position of the occipital condyles, the height of the skull above the level of the supra-orbital margin, the construction of the region of the temporo-mandibular joint, the sharp undercut margin of the lesser wing of the sphenoid bone, the contour of the palate and the body of the mandible, the relative height of the temporal squama, and so forth. It is particularly to be noted that most of these hominid features have now been demonstrated to be *consistently* present in several specimens. For example the forward position of the condyles is shown consistently in the five skull bases available, the low position of the nuchal area in the four specimens in which the occipital region is

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<sup>3</sup> 'New palaeontological evidence bearing on the evolution of the Hominoidea'. *Quart. J. Geol. Soc., London*, vol. 105, p. 225, 1950.



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preserved, and so forth. Thus, in order seriously to controvert the view that the Australopithecine skull as a whole makes a closer morphological approach to the hominid type of

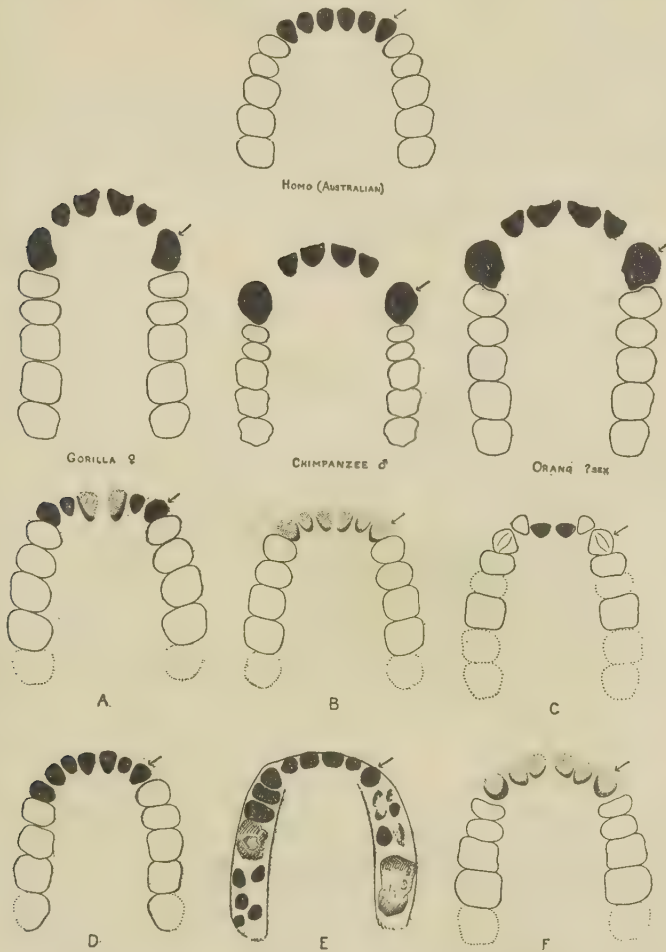


FIG. 1. THE UPPER DENTAL ARCADE OF *HOMO SAPIENS* (AUSTRALIAN NATIVE) COMPARED WITH ANTHROPOID APES (GORILLA, CHIMPANZEE AND ORANG) AND THE AUSTRALOPITHECINAE (A-F).

All at two-fifths natural size. The teeth are represented in outline, and sockets in solid black. Where any reconstruction has been necessary, this is indicated by dotted lines or stippling. Because in the fossil specimens the incisor and canine teeth are in most cases missing, they have been represented by their sockets only in the modern specimens. Note, particularly, the position and relative size of the canine or its socket (indicated by an arrow). Of the Australopithecine specimens four were found at Sterkfontein (B—type specimen, C—1936 maxilla, D—skull no. 6, and E—skull no. 5) one (A) at Kromdraai and one (F) at Makapansgat.

skull, it must be demonstrated not only that a similar total pattern of hominid features may also occur in a *single* skull of one of the anthropoid apes, but also that this pattern is consistently found in several skulls taken at random from a common group (in order to

parallel the morphological consistency shown by the several Australopithecine skulls which have now been found). With all the new material now collected by Dr Broom, this now appears to be no longer in question. And even if it were so, it would of course not be legitimate to consider the skull without reference to the dentition (or *vice versa*), and the dentition tells the same story.

The Australopithecine dentition (like the skull) displays certain hominid features which severally appear to find no precise parallel in any of the known apes (recent or extinct), and others which may very occasionally be found singly if sought in a large series. But, again, conclusions regarding the hominid nature of the dentition must ultimately be based on the total pattern determined by all these features together. Among the features referred to are the small size and spatulate form of the canine (combined with its flat wear at the tip by attrition with the opposite canine, and its orientation in the tooth row), the non-sectorial bicuspid form of the front lower premolar (with cusps of approximately equal bulk and height and well-developed anterior and posterior foveae), the evenly curved parabolic contour of the dental arcade with the *consistent* absence of any diastema (see FIG. 1), the flat type of wear of the premolars and molars in the earlier stages of attrition, and, in the temporary dentition, the shape and dimensions of the milk canine combined with the very hominid cusp pattern of the first milk molar. Dr Broom and Professor Dart have now brought to light many specimens of jaws and palates of the Australopithecinae, as well as numerous isolated teeth, and at least three excellent specimens of the milk dentition are also known. All this amazingly rich material bears witness to the fact that the same hominid pattern is quite consistently present in the Australopithecine dentition. By contrast, no similar combination of these hominid features together has been demonstrated in the dentition of any of the anthropoid apes. The implications of these observations seem clear—that in their dentition the *Australopithecinae* display a much closer resemblance to man than do any of the known anthropoid apes. Yet, in reference to a recent statistical study by Ashton and Zuckerman, the remarkable statement (4) has been made that of 75 dental characters of *Plesianthropus* none differs from the orang! As it is phrased, this statement may have the unfortunate result of leading the casual or uncritical reader unwittingly to suppose that an exhaustive statistical study has finally shown that *Plesianthropus* in its dentition is indistinguishable from an orang. This however, is certainly not so. In the first place, it has to be decided whether the characters referred to are really relevant to this particular problem of taxonomic affinities. But the fundamental question, surely, is whether the authors can produce for inspection even one sample of the dentition of an orang or other anthropoid age (let alone a whole series of individual specimens) in which the total hominid pattern consistently found in the Australopithecine dentition is reproduced, or even closely approached. Failure to do so must (it seems) render highly suspect a method of approach which, at first sight, appears to lead to conclusions so greatly at variance with the much more complete evidence which can be provided by the method of direct visual comparison.

As an example illustrating the evidence to be derived from direct visual comparison reference may be made to FIG. 1, in which are shown in outline the upper dental arcades of man, anthropoid apes, and the Australopithecinae. Now, in the first instance, a direct comparison of man and apes makes it clear that (apart from the possibility of aberrant exceptions) they show certain outstanding contrasts. Thus in man the arcade has an evenly rounded contour, the canines and incisors are relatively small, the canine is placed medially to the axis of the post-canine teeth so as to run in series with the line of the

<sup>4</sup> S. Zuckerman. *Nature*, April 22, 1950, p. 652.



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incisor teeth, and there is no conspicuous gap (diastema) between the canine and the lateral incisor. In the anthropoid apes, on the other hand, the canines and post-canine teeth form more or less straight, parallel rows, the canines and incisors are relatively large, the central incisors lie considerably in advance of the level of the canines, and there is almost invariably a well-marked diastema. The total morphological pattern presented by all these features in combination appears to distinguish man from apes rather strongly. If attention is now turned to the upper dental arcade in six different specimens of the Australopithecinae which are available (FIG. 1, A-F), it is immediately apparent that in all these specimens the arcade quite consistently resembles the hominid rather than the anthropoid

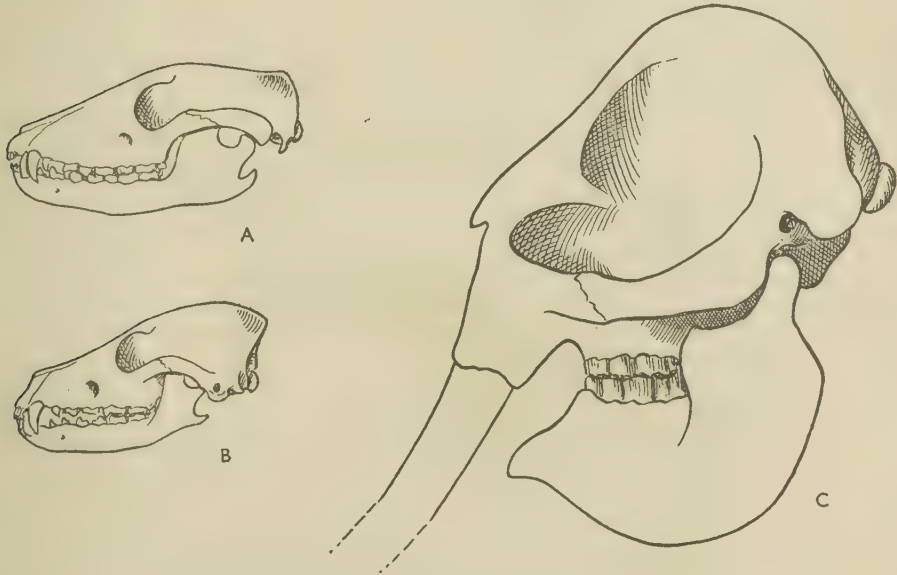


FIG. 2. A DIAGRAM ILLUSTRATING THE SKULLS OF (A) the TASMANIAN WOLF, (B) *CANIS FAMILIARIS* AND (C) THE ELEPHANT

This somewhat extreme example serves to stress the point that a statistical study confined to main dimensions and indices may (if applied without reference to the *total* morphological pattern) lead to very fallacious results in attempting by this method to assess taxonomic affinities

ape pattern, as is also the case with a number of morphological details of the teeth (*vide supra*). The question then arises—is there in fact any specimen of an ape known (even if it is quite exceptional) in which the hominid pattern is reproduced to an equivalent degree? So far as I am aware, this is not the case, and those who have been unable to recognize that in their dentition the Australopithecinae show a closer approach to hominids than do the anthropoid apes can hardly continue to maintain this view without producing such a specimen (5).

The example of the pattern of the upper dental arcade is but one of many which might be adduced in support of the evidence to be obtained by direct visual comparison.

<sup>5</sup> Indeed, as already emphasized, it would be necessary to produce several specimens, taken at random from a common group, in order to parallel the consistency with which the hominid pattern is found in the many specimens now available of the Australopithecinae.

This method, it may be noted, is no less accurate than the commonly employed method of statistical analysis of isolated metrical abstractions; it has, in addition, obvious advantages, one of which is that it can take into account many factors simultaneously. Lastly it is perhaps hardly necessary to point out that, before attempting an assessment of the affinities of the Australopithecinae, a great variety of morphological patterns need to be taken into consideration—not only the pattern of the dental arcades, but also those presented by the morphological details of the teeth, of the skull, and of the limb skeleton.

It has seemed desirable to call attention to the fallacies which may result from an uncritical application of statistical methods to too limited material in taxonomic problems, for, unless these fallacies are recognized, they may give rise to serious confusion in discussions on the significance of the Australopithecinae. That the usual statistical methods do need to be employed with extreme care and circumspection in the assessment of affinities if they are to give relevant answers may be further emphasized by reference to a somewhat extreme (but entirely apposite) example. If attention were limited to major dimensions and indices of the skulls of a Tasmanian wolf, one of the Canidae, and an elephant (see FIG. 2), the investigator might readily find a hundred or more 'characters' common only to the first two, with the implication that they are much more closely related to each other than either is to the third. But a consideration of the *total* pattern presented by the morphological details of the animal (combined with some appreciation of the real nature of morphological evidence) would be sufficient to demonstrate (what is in fact the case) that the Tasmanian wolf belongs to an entirely separate sub-class of the Mammalia (6)! A consideration of the total morphological pattern of the dentition makes it equally clear that in many of its features the Australopithecinae approach the hominid rather than anthropoid ape pattern.

Now let us turn our attention to the limb skeleton. Here again, it is clearly essential to consider total patterns rather than isolated characters. Attempts to analyse some of the metrical characters of an isolated specimen of the earlier (very fragmentary) limb bone material, for example, had led Kern and Straus (7) to the startling conclusion that the Australopithecinae might have been quadrupedal primates of a cercopithecoid type—a conclusion which was evidently inconsistent with all the other morphological evidence then available. Fortunately, Dr Broom has again clarified the issue, this time by collecting more remains of the limb skeleton, including the lower end of another femur (reproducing the hominid pattern of characters previously noted in the first specimen), and a practically complete pelvic bone. This last specimen is very remarkable indeed for, in spite of the fact that it possesses certain features in which it appears to be distinguished from *Homo sapiens* of today, in its general build and proportions it is quite clearly much more hominid than ape-like (FIG. 3). It supplies the final argument in confirmation of the South African scientists who already, some years ago, had recognized the hominid traits of the Australopithecinae and had also inferred that their posture must have approximated in some degree to that characteristic of man. It would hardly be possible now to deny this conclusion—unless, indeed, the pelvis is held to be not part of an Australopithecine skeleton at all. But surely this would be a last argument of desperation. For the pelvic bone was excavated actually *in situ* by experienced workers (and it would

<sup>6</sup> Of course, if the answer were known in advance, it might be possible to select measurements in order to demonstrate statistically that *Canis* really has closer affinities to the Elephant than to the Tasmanian wolf. But this hardly provides a vindication of this method (unless it is applied very critically).

<sup>7</sup> H. M. Kern and W. L. Straus. 'The femur of *Plesianthropus*', *Am. Journ. Phys. Anthr.*, vol. 7, p. 53, 1949.



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be difficult to rival Dr Broom's experience of over 50 years as a palaeontologist), and it was embedded in a dense matrix of stalagmitic limestone in immediate relationship to Australopithecine skulls, jaws, teeth and other elements of the limb skeleton. In their account of the Sterkfontein excavations, Broom and Robinson give a general plan of the site. This makes it clear that all the recently collected material was found within a very circumscribed area of only a few square yards, and also that the pelvis and several skulls lay at practically the same depth in the limestone matrix. It also shows that the pelvis was placed more or less centrally among the other closely packed remains, about 3 feet from one skull and less than 8 feet from another. Lastly, it is important to emphasize that, since the pelvis was found with the femur, crushed remains of vertebrae, etc., it



FIG. 3. OUTLINE DRAWINGS OF THE RIGHT PELVIC BONE (VIEWED FROM THE LATERAL ASPECT) OF (a) *PLESIANTHROPUS*, (b) CHIMPANZEE AND (c) *HOMO SAPIENS*  
(after Dr Broom)

evidently belonged to a skeleton still partly articulated at the time when it was deposited in the position in which it lay. The circumstances in which the pelvic bone was found are thus far better documented than in most fossil discoveries relating to early hominids. And even if there were any doubt on this score, there is now part of a second pelvis (the iliac bone) to provide corroborative evidence, for this specimen, which (it is important to note) reproduces the same distinctive features of the Sterkfontein pelvis, was found at a site, some 200 miles away, which has also yielded Australopithecine remains (but no trace of *Homo*).

With all the remains of the Australopithecinae which have now been discovered by Dr Broom, the real significance of these fossil primates becomes clearer. There can be no question, of course, but that they belong to the same general group which includes the Hominidae and the anthropoid apes—a group now termed the Hominoidea. There can also be no further question but that, in many respects, they show a closer morphological resemblance to the Hominidae than do any of the known anthropoid apes. The obvious implication would seem to be that they are more closely related to the Hominidae

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and, indeed, it seems a reasonable inference that they actually represent an early phase in the hominid (rather than the anthropoid ape) sequence of evolution (8). This is not to say, of course, that they are necessarily themselves ancestral to *Homo sapiens*—but at least they seem likely to be collateral descendants (not greatly modified) of this ancestral stock. Another possible interpretation, however, calls for consideration—that they are representatives of the anthropoid ape sequence which have developed an amazing parallelism to the hominid sequence (but not *directly* related to the latter). In so far as it is not possible to define the ultimate theoretical limits of parallelism in evolution, such an interpretation can perhaps not be finally excluded, but, were it true, it would surely be a very extreme example of this phenomenon. It must also be emphasized that, morphologically, there appears to be no specialized or aberrant feature of the Australopithecinae which would positively exclude them from some place in the hominid sequence. Indeed, if an attempt is made to construct a hypothetical picture of the ancestral forms which must presumably have immediately preceded the *Pithecanthropus* phase of human evolution, it is difficult to know how such ancestral forms would have differed from the Australopithecinae (except in relatively minor details).

The fact is that, if the Australopithecinae are accepted simply as an unusual example of evolutionary parallelism, the palaeontologist who is searching for a morphological precursor of *Pithecanthropus* finds himself in a very curious position indeed. For he will be driven to search in some other part of the world for fossil remains which, even if found, would not show any really fundamental differences from the South African Australopithecinae which have already been discovered by Dr Broom. And then, of course, he would still have to face the captious criticism that his new discoveries are also nothing more than extreme examples of parallelism !

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<sup>8</sup> The arguments in favour of this interpretation have been presented in some detail elsewhere (*Quart. J. Geol. Soc., London*, vol. 105, p. 225, 1950).



# George Grant MacCurdy

by EARNEST A. HOOTON

*Harvard University*

THIS appreciation of George Grant MacCurdy does not purport to record either the main facts or the details of his long and fruitful life. It is merely the appraisal of the man, the teacher, and the scientist by one who was his junior friend and colleague for more than a third of a century. It is designed to supplement the admirable biographical sketch written by Dr Hugh Hencken for the *Bulletin* §16 of the *American School of Prehistoric Research*.

Thirty-five years ago, Dr MacCurdy and his friend, Dr Charles Peabody, then Curator of European Archaeology in the Peabody Museum of Harvard University, were practically the only American scholars conversant with the field of European Prehistory and known and recognized by workers in that field. Dr MacCurdy, who kept himself completely abreast of scientific contributions to the subject, served as the principal medium of disseminating this knowledge to American anthropologists, by numerous clear and succinct accounts and reviews of the literature, mostly published in the *American Anthropologist*. He had himself produced the outstanding monograph in English on the subject of Eoliths, which is still worthy of perusal. The idea of the American School of Prehistoric Research was originated jointly by MacCurdy and Peabody, but, after the first season or two, Dr MacCurdy carried the whole burden and responsibility of the project. The School received at this time very little support or encouragement from the American Anthropological Association or from professional anthropological workers in this country. Nels Nelson of the American Museum of Natural History and the writer of this account, together with the late Dr Hrdlička of the U.S. National Museum, were almost the only American anthropologists to manifest an active interest. Nevertheless, Dr MacCurdy with the indispensable and incalculably valuable assistance of his dynamic wife, persisted in the difficult undertaking. American students of Prehistory and of Physical Anthropology who looked beyond the New World horizon, began to flock to the summer field and laboratory session of the School, where they received from Dr MacCurdy and from the many European authorities who were his eager collaborators basic instruction in the field and a first-hand acquaintance with the great collections in European Museums. They also had the opportunity to participate in excavations in many countries of Europe. The results of this MacCurdy effort was the awakening of professional interest and the establishment of courses in European Prehistory in many institutions of this country by the former students of the School.

At the same time, Dr MacCurdy recognized the importance of acquainting educated and intelligent laymen with the fascinating discoveries in the Old World of prehistoric art and culture. One of the first and most important converts of Dr MacCurdy to an enthusiasm for prehistory was the late Addison L. Greene, of Holyoke, Mass., for many years Chairman of the Board of Trustees of the School, and an indefatigable and successful promoter of its interests. Another important addition to the field of amateur prehistorians was General Charles G. Dawes. It was Dr MacCurdy's practice to take with him on the summer sessions of the School a few of these influential and cultured Americans and to indoctrinate them with his own zeal for prehistoric studies by actually

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taking them to the sites and showing them the ancient works of art and industry. In this effort Dr MacCurdy was pre-eminently successful, largely because of his ability to make things clear to the uninstructed layman, but even more through his own obviously pure and disinterested scientific zeal. While professional anthropological sceptics stood aside and scoffed, Dr MacCurdy laid a substantial foundation for prehistoric studies among American graduate students, raised up new experts in the field from the ranks of young people who had received no previous anthropological instruction of any kind, made helpful friends for the School among persons of maturity who had already achieved distinction in business, in professions, or in statesmanship. At the same time Dr MacCurdy enormously stimulated the actual field work of European archaeologists by giving them assistance in the way of grants from the School, in return for which they taught his students and allowed them to participate in actual excavations.

In 1925 Dr MacCurdy gave a further impulse to studies of Prehistory by the publication of his great work—*Human Origins*—which has not been superseded up to this day by any treatise on the subject of comparable authority and scope.

The short summer excavations, the museum visits, and the evangelizing of adult lay interest were soon accompanied by the institution of many important and elaborate excavation projects, carried on by advanced students of the School in Europe and elsewhere. Notable were the many careful excavations carried on in Central and South-eastern Europe by the late Dr Vladimir Fewkes and his assistants, and the joint effort with the British School of Archaeology at Mt. Carmel, Palestine, where, under the leadership of Professor Dorothy Garrod, the tremendous discoveries of Neanderthal Man at Mt. Carmel were made. It is perhaps safe to state that no enterprise during the present century has contributed more to the advancement of prehistoric studies in Europe than have the multifarious activities of the American School of Prehistoric Research, all activated by Dr MacCurdy.

George Grant MacCurdy was one of the most unselfish, kindly and tolerant of men. He was wholly incapable of self-promotion and congenitally non-political in his behaviour. While he was able to achieve marvels in the development of his scientific child, the American School, he neither desired nor sought scientific personal distinction and academic preferment. However, he achieved among all who knew him well a reputation for thorough scholarship and skilled teaching that could be envied by any class-room lecturer on anthropological subjects. Moreover, the character of the man was such that meanness, malice, and uncharitableness were wholly alien to him. He was a great gentleman, revered and beloved by his students.

The American School of Prehistoric Research is Dr MacCurdy's monument and should perpetuate his memory, not necessarily in name or in details of present organization, but in its continuing contributions to our knowledge of ancient man and in its raising up of new generations of prehistorians and of amateurs of the study of man.



# Jarmo: A Village of Early Farmers in Iraq

by ROBERT J. BRAIDWOOD

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THERE is a body of theory, most clearly delineated in recent years by V. Gordon Childe, concerning the importance of the appearance of Food-Production as a basic economic revolution. The domestication of plants and animals assured a stable food supply. Proper village life now came into being, and with it a completely new kind of technology. This latter depends on the fact that time now became available for pursuits other than that of simply collecting food. The theory holds it to be no coincidence that such crafts as architecture, pottery, weaving, and presently metallurgy make their appearance with the establishment of Food-production. These crafts make use of materials constructively, and in some cases actually change the physical or chemical properties of the materials. Such a technology was not characteristic of the preceding Food-gathering stage. The Food-producing revolution and the type of technology which attended it were at least the economic prerequisite for the appearance of *civilization* (in any useful sense of that word).

In spite of the relatively imprecise chronology now available to pre- and proto-historic archaeology it is possible to maintain that Food-production made its first appearance in the Near-middle Eastern area perhaps as early as eight thousand years ago. *Civilization*—characterized among other things by urbanization and the formal political state—also made its appearance first in the Near East at c. 3000 B.C. There is at least one other independent appearance of Food-production—and of subsequent *civilization*—somewhat later in the New World. The evidence remains unclear as to whether independent beginnings in Food-production were made in eastern or south-eastern Asia.

The actual origins of Food-production in the Near East (as elsewhere) are still unclear from the available archaeological evidence. The earliest *clearly defined* village assemblages in the Near East, which are *now in hand*, would seem to be five: that represented by Sialk I in north central Iran, by Hassuna and Nineveh I in northerly Iraq, by the basal Amouq materials and the 'Neolithic' of Mersin in Syro-Cilicia, by Jericho 9 (1) in inland Palestine, and by Fayum A in Lower Egypt. This assessment is made on the grounds of materials available to us or adequately published; we assume that the relatively comparable and simple technology of these five assemblages indicates their chronological contemporaneity in a rough way. We have consciously not included the Tasian of Upper Egypt in the above picture, as we feel it *may* simply be a typological

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<sup>1</sup> And Jericho levels 10-17 as well? The flints, at least, seem consistent within the whole range from Jericho 17 through 9. And what indications are available of architectural plan seem also to be consistent throughout some of these levels including 9.

construct of 'impoverished' Badarian materials. We believe there may be snatches of other assemblages of a date and technological simplicity comparable to the above mentioned five (for example Bakun B-1) but one cannot get at them as yet.

What clearly defined materials precede the available five earliest village assemblages (Sialk 1, Hassuna, Amouq-Mersin, Jericho 9, and Fayum A)? Looking backwards in time from the stage they represent, we see no *clearly defined* materials lying between them and the Natufian of Palestine. Whether the Natufian assemblage is a terminal Food-gathering one (as we tend to believe) or an incipient agricultural stage (as Childe and others hold) is mainly a matter of semantics. We had already in 1941 postulated a gap in archaeological knowledge between the stage represented by the Natufian and that of the five earliest available villages. To our way of thinking, the supposed continuity between the Natufian and basal Jericho (whether by way of the nebulous 'Tahunian' or not) is a mirage. We believe Abu Usba may suggest a coastal Palestinian early village assemblage (rather than the continuum of the Natufian the excavator suggests) distinct from Jericho. So far there is not enough of it available to go beyond the point of suggestion. There are a few other scraps of material but we are convinced a gap in knowledge exists between the Natufian and the earliest available villages. We also reason that it would be exactly within the time range covered by this gap in knowledge that the essential elements of the Food-producing revolution came into being.

This reasoning pointed the problem for our first field work for the Oriental Institute following the war. The five earliest available village assemblages (Fayum making somewhat of an exception) lie on the hilly flanks of Breasted's 'Fertile Crescent': those in western Asia are all above the present 250 mm. or even the 500 mm. isoyhet. We decided to seek a site rather near the apex of the hilly flanks of the crescent in Iraq. Our application to the Iraqi Directorate General of Antiquities was met with enthusiasm. Dr Naji al-Asil, the Director General, provided us with a list and recommendations for some fifty promising sites of prehistoric villages which his staff had noted on survey. We chose for excavation a site called Matarrah, some 34 kms. south of Kirkuk, which had predominantly Hassunan surface materials and which lay well to the south of the type site. We also asked for a sounding permit on a second site, called Qalat Jarmo, midway between Kirkuk and Suleimaniyah. We could not at long range anticipate the full promise of Jarmo. Messrs Seton Lloyd and Fuad Safar had visited it and made surface collections which were reported to consist only of flaked flint tools including some of microlithic size, and a few bits of ground stone. The site was thus assessed as 'Mesolithic' and until we were able to test it, we were not certain whether it would prove fully pertinent to our central problem. The entire credit for locating Jarmo and for realizing its potential interest for pre- and protohistoric archaeology belongs to the Iraqi Directorate General of Antiquities.

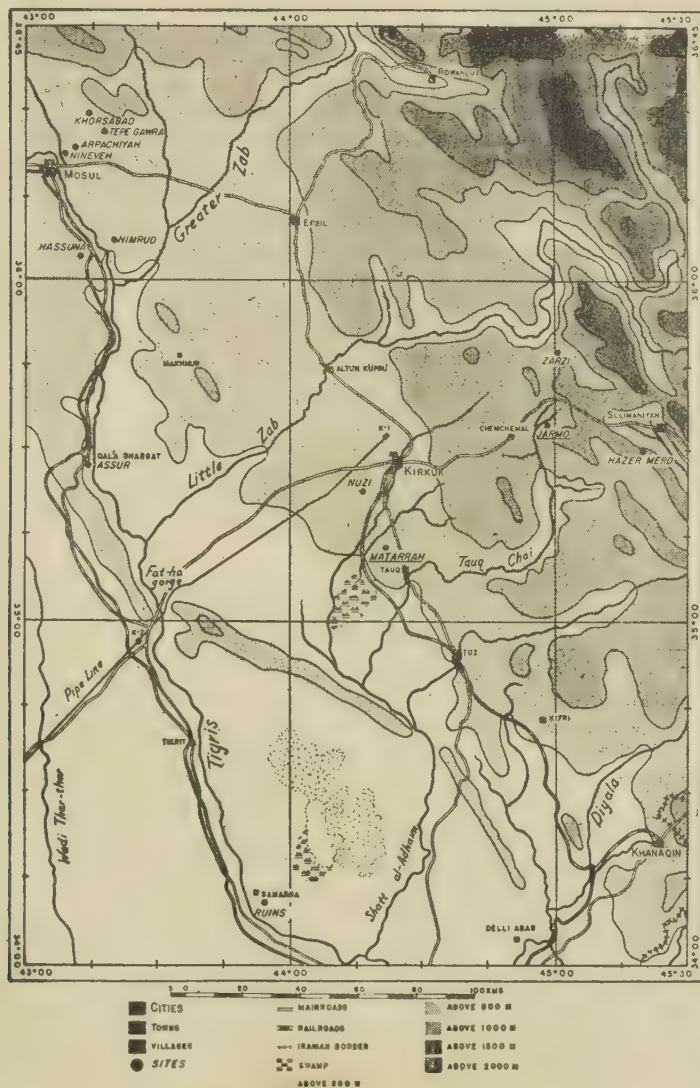
The site at Matarrah yielded a southern variant of the Hassuna assemblage, which is to be reported on in an early issue of the *Journal of Near Eastern Studies*. It did not fulfil our expectations that it might contain pre-Hassunan materials. As to Jarmo, the combination of a one month sounding-permit and the approach of hot weather forced the campaign at Jarmo to be a brief one. We are fortunate at this writing to be *en route* to Jarmo again for a full season's work there (2).

<sup>2</sup> The staff during our 1948 campaign at Matarrah and Jarmo consisted of Dr Faraj Basmachi of the Directorate General of Antiquities, Miss Charlotte Otten, a graduate student on grant from the Department of Anthropology of the University of Chicago, and ourselves for the Oriental Institute. We are deeply indebted to the Director General and all of his staff for their cooperation, and to our colleagues in Anthropology for enabling Miss Otten to be with us.



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Jarmo lies on a kind of natural promontory formed by deep *wadi* cuttings. The landscape is one of an upland grassy terrace of top soil on decomposing sandstone, sloping off from the Sagirma ridge to the east, with interruptions by the deep ravines of the



*wadis*. The mound debris of the site itself is some 5.0 m. in thickness over the sandstone of the original terrace at the point where we tested it. The debris by surface indication seems to cover an ovoid area of *c.* 90.0 by 140.0 m., which makes up most of

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the promontory. The west, north, and northeast slopes of the promontory are quite steep and fall *c.* 50.0 m. to the *wadi* bed. There is a perennial spring in the *wadi* several hundred yards northeast of the promontory.

Save for three shallow 2.0 by 2.0 m. testpits, our major effort on Jarmo in the month of May, 1948, was a 7.0 by 10.0 m. area taken down to *c.* 3.5 m. over the whole area, then reduced by half, and cleared to bed-rock at *c.* 5.0 m. depth in only about 8 square metres. Within this depth there were eight more-or-less well-marked floor levels, and hints that several more intermediate levels might develop further into the core of the mound. The assemblage which these levels yielded was essentially consistent throughout the total depth; the exception to this came in the matter of potsherds.

A meagre quantity of coarse hand-made pottery appeared in the uppermost 75 cm. in our operations at Jarmo. This is, in effect, the depth of the present root line. One or two small and fragmentary patches of cobble-stone paving or wall founding appeared within this depth, and also several fragments of what had apparently been bitumen surfaced 'hearths.' Fragments of laid stone, and of bitumen flooring appeared below the 75 cm. depth, however, and there was absolutely nothing in any other category of artifacts in the uppermost 75 cm. depth which was inconsistent with the materials below, save for the potsherds. The sherds themselves were of small size, with badly eroded edges; lip sections were available and were very simply profiled. Most of the sherds showed traces of chaff temper; they appeared to be incompletely fired and their surfaces were somewhat fugitive when we washed them. Perhaps 25 per cent of the bulk showed traces of the addition of a red surface solution, and burnish marks could occasionally be noticed. On the whole, the sherds were too featureless to allow us to compare them with any other early pottery of our acquaintance.

There was very little trace of architecture in the next *c.* 1.5 to 2.0 m. depth of deposit in our main exposure. Incomplete runs of *touf* (i.e., *pisé* or loaded mud) walling with a corner or two appeared, assuring only that their original structures had been rectilinear. At *c.* 3.5 m. depth, the upper parts of the wall butts of a building in level 6 began to appear. This was not complete in the area we exposed, but consisted of at least two fair sized rooms and suggestions of two or three more rooms. There were connecting doors, and the floor of one room bore the traces of reeds—laid lengthwise, as if to form a simple bed. Querns and hand grinders appeared in two of the rooms. At a slightly lower level (presumably a level which will lie between the levels 6 and 7 we first identified) was the trace of another reed floored 'bedroom.' We encountered no architecture in the restricted portions of levels 7 and 8 which we exposed.

At level 5, which had fragments of walls, but which was not well preserved architecturally, we found approximately two-thirds of what had apparently been an elliptical baked-in-place clay basin, of perhaps 50 cms. at its greatest diameter. This appeared to have been built by simply scooping a basin-shaped hole in the floor, facing it with clay, and building a fire in it. The characteristic sherds of these unportable basins (surfaced on one face only) appeared at all depths in our exposure, and there were two other smaller *in situ* fragments. These of course recall the basins Professor Garstang noted in Jericho 9, save that at Jarmo they run through a succession of levels. We did not recover any fragments of these basins which could be called rim-sherds.

There were practically no other utilitarian (to our mind) objects of baked clay in the Jarmo assemblage. Small clay cones and bulla-like blobs of lightly baked or unbaked clay appeared in quantity. There were simple clay beads and a number of figurines. These were predominantly of animals but there were also convincing representations of



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human heads and of a seated type of mother goddess. Only a small portion of these figurines had been fully fired.

The lack of spindle whorls in either clay or stone might be noted. There were, however, fragmentary impressions of simple over-and-under reed mats, and also of a somewhat more complicated type of mat or basket weave—the ghost of this latter is not completely clear.

In the flaked stone category, the situation at level 6 can probably be taken as typical. Of the total number of flints and obsidian found in level 6 (*c.* 4000 including worked examples and mere chips) about 40 per cent are normal or large size artifacts and roughly 60 per cent are of a microlithic size. Obsidian was used in a fair quantity but in the main only for microliths. The industry is predominantly a blade tool industry. The retouch is neat. The tool kit seems to be very limited. The larger size tools include sickle blades, borers, and scrapers. The microliths are fairly monotonous in that the majority are neat, sharply pointed blades, used without further retouch. The small proportion that has been retouched is mainly borers but also includes an occasional blunted back blade. Geometric microliths, except for five or six examples scattered throughout the levels, are non-existent.

It is pertinent to point out here that Jarmo lies only 15 miles from Professor Garrod's cave at Zarzi. Zarzi seemed to her to be an extended Gravettian industry with a microlithic complement. The Zarzi microliths even though few in quantity show a variety of types not found in the simple Jarmo repertoire. It would seem that despite the great abundance of microliths at Jarmo, their intrinsic monotony, taken in conjunction with the rest of the Jarmo assemblage, suggests a tapering off of interest in tools in the microlithic category.

There is considerable variety in ground stone tools. The presence of querns and hand-rubbers has been noted, and there were also boulder mortars and pestles. Included under the name of 'pestles' were numbers of perfectly shaped small and even minute pieces—we do not know their utility. There was a flourishing industry in finely ground stone bowls; as with the small pestle-like pieces, considerable care seems to have been given to the selection of handsome varieties of stone, and to the cutting of the bowls so that the coloured veins of marble-like stones would appear as banding on the bowl. Full ground celts were available but in no great number; both axes and adzes are present. There were large sized pierced oblates of stone which we suppose may have been dibble-weights. Under the non-utilitarian ground stone category came a phallus and a variety of beads mainly simple but including a few more elaborate forms such as a long bead of flattened lentoid section that was collared. There were also many bracelet fragments usually of marble, some with grooved decoration. Some of these also had holes for segmental binding. There were a few animal figurines in stone and also several stone 'pegs' or 'nails.'

The bone category was rather simple; it included articular butted or broken butted awls, fragmentary rib-blades, delicate needles, and gorget fragments. There was also a peculiar sort of shaft-hole double gouge-like tool, and a long handled, shallow bowled spoon.

In shell, we noted only a fragmentary double pierced pendant.

Much of the interest of the catalogue of Jarmo materials lies in the non-artifactual materials. Such grain as we did find was badly carbonized; it was given a preliminary identification as both 'wheat' and 'barley' by botanists in the United States Department of Agriculture, and has now gone into the hands of Professor Paul Mangelsdorf of the Department of Botany at Harvard University for further study. The animal bones were

given over to Bryan Patterson, Curator of Vertebrate Paleontology of the Chicago Natural History Museum. Dr Patterson's preliminary study has gone so far as to indicate the presence of sheep and/or goat, cattle, pig, dog, and several equid teeth. Specific recognition has so far not been completed but Dr Patterson is of the opinion that c. 95 per cent of the bulk of the Jarmo faunal remains fall in the above categories, and that if these beasts were not 'domesticated' in the full sense of the word, they were at least 'potentially domesticable.' Wild animal forms made up the remaining 5 per cent of the bulk. In all levels at Jarmo but especially concentrated in levels 7 and 8 there were land snails. A number of these were given over to Fritz Haas, Curator of Lower Invertebrates of the Chicago Natural History Museum. A specific recognition is not yet available, but Dr Haas is of the opinion that the ecological situation suggested by these snails would be one of open grass lands (such as now obtains at Jarmo).

There were several flexed sub-floor burials at Jarmo. No grave-goods appeared with them. The skeletons were themselves in too fragmentary a condition for restoration, but the teeth have been given over to Dr Albert Dahlberg, Research Associate of the Department of Anthropology of the University of Chicago for further study.

We were particularly fortunate in having a sufficient quantity of the Jarmo levels 7-8 snail shells for a single 'run' by the Carbon 14 dating project, under Professor W. F. Libby of the Institute for Nuclear Studies of the University of Chicago. Dr Libby and his colleague, Dr James Arnold, have remained somewhat restless about the reliability of shell, especially when only one 'run' is available, and when there is no check with a vegetable carbon material. Hence the result can only stand as tentative. It is  $6900 \pm 320$  years ago, or within the range from 5270 B.C. to 4630 B.C. (subject to the theory of probable error as Dr Libby applies it).

Jarmo has by no means closed the gap in archaeological knowledge between the stage represented by the Natufian of Palestine and the five available earliest village assemblages. On typological grounds Jarmo is certainly pre-Hassunan but post-Natufian. In fact it is probably much closer to the Hassunan than to the Natufian stage, typologically speaking. It does not, however, have as many traits which would clearly suggest themselves as antecedents for Hassunan traits as one might have suspected. This generalization may, of course, need revision when a larger exposure is made at Jarmo.

We are quite conscious of the fact that Jarmo is up in the hills, well above the typical Iraqi plain wherein Hassuna, Nineveh, and Matarrah lie. This raises the question of whether it might be a 'back-water' culturally speaking, and hence not chronologically earlier than the Hassunan assemblage. We do not think this is true, nor did those of our colleagues who visited the site. The question must, however, remain open until both the Jarmo and Hassuna stages can be adequately fixed by Carbon 14 or until an assemblage intermediate to Jarmo and Hassuna is discovered and intercalated between the two.

Jarmo has also stimulated our interest to know more of Jericho 10-17. Save for the excellent treatment of the flints, this range of Jericho has been a depressing one to handle, due to inadequacy of publication. It seems to us quite probable that it represents a stage similar to that of Jarmo. Since 1948 several other sites have been worked, which—when details concerning them become available—may also prove to fit somewhere into the general picture. These are the cave sites worked by Professor Carleton Coon on the Caspian terraces in northern Iran, by M. Roman Ghirshman in the Bakhtiari hills in Iran, and M. Mosé Stekelis on the Yarmuk in Palestine.

A final observation, however subjective, came to us through handling the Jarmo



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materials at the same time as we were processing those of the Hassunan stage Matarrah site. What struck us was the contrast between the 'feel' or 'character' of the various artifacts from Jarmo, as against those of Matarrah. It was a contrast of dynamic with static, so to speak ; as if the Jarmo craftsmen had been in a phase of productive intensity and creativity, whereas the Matarrah craftsmen were simply producing—in a rather spiritless way, as if they had reached a peak and then levelled off.

We would like to believe that this observation does suggest something. Even though Jarmo does not by any means close the gap in archaeological knowledge, we suppose it to be the most substantial assemblage now available *nearest* to the beginnings of Food-production. When Food-production first came into being, there must have been a remarkable release of cultural potential, just as there was following the time of James Watt and the beginnings of Industrialization. In this sense, the burst of technological activity which Jarmo seems to indicate may roughly parallel that of our own times (3). But as is usual in prehistoric archaeology, a considerably greater bulk of material than is now in hand must be reclaimed and interpreted. When this is done, the rest of the picture—in its sociological, religious, aesthetic, etc., aspects—will become as clear as archaeology can ever make it.

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<sup>3</sup> This does not necessarily mean that Jarmo stands just two centuries following the beginnings of Food-production, as we do from the beginnings of Industrialization. The actual rate of acceleration was probably slower. But before Dr Libby's date became available, we had guessed that Jarmo might have an antiquity of *c.* 6000 B.C. The later Carbon date does not depress us, however. It suggests rather that once Food-production came into being, the rate of cultural acceleration—relative to those times—was as remarkable as the rapid technological advances of our own.

# Palestinian Excavations

by KATHLEEN M. KENYON

BETH-SHAN. Vol. II. Four Canaanite Temples. Pt. I. The Temples and Cult Objects. Alan Rowe. Philadelphia: published for the University Museum by the University of Pennsylvania Press. \$15. XII+101 pp., LXXI plates. 1940.

MEGIDDO II. Seasons of 1935-9. Gordon Loud. Published by the University of Chicago Press as the University of Chicago Oriental Institute Publications, Vol. LXII. 1 vol. of text, XXI+199 pp., 416 text figures, 1 vol. plates, 91 plates line blocks, 199 plates photographs, with accompanying description. 1948.

EXCAVATIONS AT TELL EN-NASBEH. Published by the Palestine Institute of Pacific School of Religion and the American Schools of Oriental Research. Vol. I, Archaeological and Historical Results, by C. C. McCown. XXII+322 pp., 112 plates. Vol. II, The Pottery, by I. C. Wampler. 186 pp., 90 plates. 1947.

RECENT years have seen the publication of some sumptuous reports of the large-scale excavations conducted by American enterprise in the years before the war. The sites of Megiddo and Beth-Shan, which can confidently be referred to by their historical and biblical names, are of outstanding importance, dominating as they do the Plain of Esdraelon and the great road from Egypt to North Syria and Mesopotamia. The publications here considered are the latest (but not, it is hoped, the last) of a series dealing with different aspects of the excavations. Both sites have been partially sounded to bed-rock, and show continuous occupation from the chalcolithic period to the end of the first millennium B.C., and Beth-Shan beyond it. Tell en-Nasbeh is in a different category. It is possibly to be identified with the Biblical Mizpah, but this is not universally accepted. Like many Palestinian hill-country sites, it was occupied in the Early Bronze Age (the ascription of some groups to the chalcolithic period is unsatisfactory). Its main occupation is, however, confined to the Early Iron Age, from the time of the undivided Israelite Kingdom down to the post-exilic, Hellenistic and Roman periods.

The Megiddo excavations are the better and more completely published and probably the better executed. The present volume is concerned mainly with strata XX (the lowest) to VI, with some additional matter on strata V and IV, already fully published in *Megiddo I*. The pottery of strata XX to VI has already been summarily but most usefully published (1). It is here re-published with full descriptions and adequate scale drawings, and it is satisfactory that an interim publication has not tempted those responsible to scamp the full publication, a failing which besets so many excavators.

The foreword emphasizes the fact that neither the excavations nor the publication are as complete as had originally been intended. The original plan had been to excavate every stratum from top to bottom of the Tell completely, an undertaking which must have taxed the resources even of the Oriental Institute in its prime (the area of the summit is 13 acres, the base being considerably larger, and the maximum depth of deposit some 20 metres). Such a scheme bears tribute to the breadth of vision of the late J. H. Breasted, but it may be doubted whether it is a wise one. In England at the present time emphasis

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<sup>1</sup> Shipton, 'Notes on the Megiddo Pottery of Strata VI-XX'. No. 17 of *Studies in Ancient Oriental Civilization*, published by the Oriental Institute of the University of Chicago.

is being laid on the necessity of complete horizontal excavation on selected sites. But such sites should be of manageable size and preferably of restricted period. It is very questionable whether key sites, giving important successions of culture, should be completely excavated. Excavation technique is, and should be, always progressing, and sections of the site should be left for the findings of the earlier excavations to be checked and amplified by later methods. The original Megiddo scheme was, however, abandoned when stratum v was reached, owing to the constriction of the resources of the Oriental Institute in the economic depression of the thirties, and even the more modest scheme to excavate to bed-rock in certain sectors only was not completed in various planned linkings-up, owing to the war.

The war was also responsible for the publication of *Megiddo II* being much less exhaustive than that of *Megiddo I*. The authors only claim that it is 'a catalogue of the architecture and artifacts recovered primarily in the seasons of 1935-39'. No attempt is made to give comparative material nor to discuss implications. It was felt that the steady withdrawal of members of the staff for war-work demanded a choice between this type of publication immediately, or indefinite delay. The choice was probably rightly made. Users of the volume have to make their own comparisons and conclusions, but at least the material is fully available.

In a review, only a few of the most salient points of importance can be noted. Perhaps the most outstanding is the contribution knowledge of culture and of chronology in Palestine in the fourth and third millennia B.C., which comes from one cutting to bed-rock (BB) and a separately published (2) clearance (for the purpose of dumping) on the slopes, called Megiddo Stages VII to I. The earliest occupation (-xx) was found to be characterized by a flint industry of the Canaanite type, which was shown at Jericho to differ from the Neolithic type of Jericho IX, and to appear in the Chalcolithic (I prefer Garstang's original nomenclature) Jericho VIII, continuing throughout the Early Bronze Age. In Megiddo xx, the same flint industry (with only slight differences) is accompanied by pottery, of a type linked to (though not identical with) Ghassul IV, Jericho VIII, and Beth-Shan XVIII. The particular interest of this lies in the fact of the appearance of the manufacture of pottery at two different stages in Palestine. At Jericho it appears in IX, a level characterized by a neolithic Tahurian flint industry, and, as at Megiddo, going back to before the appearance of pottery. There is probably a gap at Jericho between IX and VIII, representing most of the occupation at Ghassul, and the above mentioned parallelism between Megiddo, Ghassul and Jericho makes it clear that the first pottery level at Megiddo is very considerably later, and that it belongs to a cultural phase different from this event at Jericho. The evidence therefore suggests, though it needs confirmation, that pot-making was an indigenous product of Jericho (for which there is other suggestive evidence), and was not generally introduced in Palestine (for Megiddo is unlikely to be a backward site) until considerably later.

From Megiddo xx onwards, there is an unbroken succession right down to historic times. The evidence from Megiddo and Jericho in combination provides useful pegs for joining up Palestine with Egypt and Mesopotamia, and therefore with fixed chronology. The details of the evidence cannot be given here, but may be summarized as follows: Early Bronze Ia and Ib equals late pre-Dynastic period in Egypt and Mesopotamia; E.B. II equals Egyptian Old Empire; E.B. III equals Pyramid Age of Egypt; the degenerate E.B. IV and the succeeding period, called by Albright Middle Bronze I, but more satisfactorily called Intermediate E.B.-M.B., since it is an intrusive

<sup>2</sup> Ensberg and Shipton, 'Notes on the Early Pottery of Megiddo'.



culture quite unrelated to the subsequent Middle Bronze culture, equals Egyptian 1st Intermediate.

The importance of the Megiddo publication, therefore, needs no emphasizing. The publication is lavish, and the documentation for the most part adequate. The lack of evidence about stratification is the weakest point of the publication. The only published sections are of the primitive type with walls floating in air, occasionally joined by dotted lines, with no record of the *actual* strata. The reader has therefore no data for assessing the correctness of the attributions to the different strata.

The publication of the Four Canaanite Temples of Beth-Shan was preceded by the publication of the pottery from these levels (3) which must here be considered with it. The four temples are assigned to the periods of Amenophis III, Seti I and two to that of Rameses III. This is on the grounds of the discovery of inscriptions or scarabs of these rulers in the associated levels. Weight must of course be given to such finds, certainly as *termini post quos* and as absolute dating if they are certainly in position. But in the instances under consideration this is not the case, the stelae not being *in situ* and the scarabs, though some were in foundation deposits, may, as Albright suggests, have been out-of-date *ex votos*. There is therefore clearly a risk that stelae may be purposely transferred from one level to another, and of small objects such as scarabs being in fact, in debris, or alternatively being treasured as valuables over a considerable period. This is actually shown by the fact that between the publication of *Beth-Shan* II.11 and *Beth-Shan* II.1, the level previously ascribed to Rameses II on the basis of finds of scarabs, has been brought down to Rameses III in view of the discovery of an inscription. Therefore it is necessary to consider the evidence, confirmatory or otherwise, of the pottery and other datable finds. A survey of this evidence leaves no doubt that the periods assigned must be very considerably modified.

This is not the place to discuss in detail the comparisons which suggest a revision of dates; the need for them is generally admitted, and it is hoped to publish such a discussion soon elsewhere. It must be admitted that much additional evidence has appeared since the publication of the pottery report in 1930 and some of it since the report on the structures appeared in 1940. On this evidence, from Megiddo, Tell Abu Hawam, Tell Beit Mirsim and Samaria, it would appear that the 'pre-Amenophis III, 1447-1412 B.C.' level should be dated to 1350-1300 B.C.; the 'Amenophis III to Horemheb, 1411-1314 B.C.' temple to 1300-1150 B.C.; the 'Early Seti I' level to 1150-1100 B.C.; the 'Late Seti I' level to 1150-1000 B.C.; and the 'Rameses III' temples to 1000-850 B.C.

The publication of *Beth-Shan* II.1 is thorough. The architectural details are fully discussed, and it is interesting to see the extent of Egyptian influence in earlier temples, revealed also by many of the objects found. The religion practised was clearly the syncretistic affair general in Palestine in the Bronze Age, but the worship of Ashtoreth, with strong chthonic tendencies, played the dominant part, perhaps in association with that of a local warrior god Mikal, later assimilated to Resheph and superseded by the Philistine god Dagon. The cult objects were numerous, and are well published and exhaustively discussed. But as at Megiddo, the stratigraphy is inadequate. The section through the Tell (Pl. I) is entirely schematic, and those through the Temples, though it is refreshing to find them, inadequate and unrealistic.

It is, however, in the excavation of Tell en-Nasbeh that the most deplorable absence of stratigraphical digging is evident. One must sympathize deeply with anyone who has to prepare a publication after the death of the excavator. The authors have made in

<sup>3</sup> *The Four Canaanite Temples of Beth-Shan*. Pt. II. 'The Pottery'. G. M. Fitzgerald. Philadelphia, 1930.

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many ways a magnificent job of it, but one's sympathy is tempered by observing that they do not realize the true inadequacy of the material upon which they have to work. Numerous statements are made throughout the volumes such as 'there was no clear stratification during a good part of the period covered because there was no complete destruction and rebuilding at any one time' (Vol. I, p. 10).

It must be confessed that the excavation of Tell en-Nasbeh is an outstanding example of a belief which should have died out in the nineteenth century, that a Professor of Classics, with no field experience, is capable of excavating a Roman site. It was considered that a Professor of Old Testament Literature and Semitic Language was, as such, competent to direct a large scale Palestinian dig. One would not wish to be unkind to the much-respected memory of Dr Badé, but the matter is of such importance to the future of Palestinian Archaeology that the criticism must be made. It is still more important from the fact that Dr Badé did indeed pay much attention to excavation technique, and even as the result of his Tell en-Nasbeh experience produced a Manual of Excavation. He was fully alive to the importance of the exact and complete recording of finds. But this, without an understanding of stratigraphy in the modern sense, is worthless. The result is that everything in the Tell dig is recorded by *locus*, which mostly means a room of the first-reached structure. All the baskets were numbered, counted, sorted and indexed, but digging apparently proceeded from the top to the bottom of the *locus* without reference to the relation of the layers of the soil to the structure. It need not therefore be said that in fact there is no real evidence for dating any structure, only a balance of probabilities. Pl. 3.2 is an almost perfect example of how not to excavate—an untidy trench along the face of the city wall, cutting all stratification running up to it.

The publication is extremely thorough; 59 pages are devoted to the discussion of the identification of the site with the Biblical Mizpah. The only result is to show that there is nothing conclusively against it; one school of philologists says emphatically that it is impossible for the name Mizpah to develop into Nasbeh, while another says that it is quite possible.

The discussion of the buildings is almost entirely vitiated by the lack of chronological evidence about them. The plan does show an apparently interesting town lay-out. But 'it is often impossible to determine whether, on the plans, walls of more than one period may not have been combined [so as] to give an illusory appearance of crowding' (Vol. I, p. 228). Therefore, it is useless to spend time discussing it, though reference must be made to the interesting so-called 'four-roomed building' (p. 206 ff) in which it is clear that in at least two cases the supposed three parallel rooms (with the fourth across the end) are in fact a single, aisled, room, with piers (surviving completely in one case and partially in another) resting on sleeper walls. A house basement 1.10 m. high, again, is surely inadequate as a 'shelter for asses, sheep, and goats' (Vol. I, p. 213). But there is enough to show that a great opportunity has been lost on a site entirely suitable (see p. 197 above) for complete excavation.

The publication of the pottery and other finds is very full, but again much of it loses usefulness from the lack of observed stratification. The reproduction of the photographs by collotype is not a satisfactory medium. There are, however, tomb and cistern groups which are valuable, and their date is very fully discussed. The excavator of a Near Eastern site has always to make a difficult choice between the publication of a type series and that of groups as such, with inevitable repetition. Probably the type series is the better solution (if both cannot be done), but it must be thoroughly cross-referenced, which is not done here, for there is nowhere discoverable a full list of the types occurring in any one tomb.

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An immense amount of labour has been expended on the discussion of the date of the tombs, but the result is unnecessarily confusing. An elaborate statistical method has been adopted. All parallels in form to a pot-type have been traced, including M.B. ones to a clearly E.I. pot (differences in technique being ignored). The result, as a graph of frequency, is plotted for the tomb, in combination of the frequency graphs of other forms. This gives a series of peaks which more or less coincide, but gives the ludicrous result that the extremes of the graph stretch from the seventeenth to the second centuries B.C. On the other hand, no account is taken of the appearance or non-appearance of diagnostic forms or techniques, on the basis of which one need have little hesitation in giving terminal dates of *c.* 900 B.C. to Tomb 54, *c.* 850 B.C. to Tomb 32 (the identification of the scarabs as 25th Dynasty is doubtful, and they must here belong to an earlier, similar, group of which the existence has already been suspected) and probably to Tomb 5, and *c.* 800 B.C. to Tomb 29.

These three publications have made it possible to review the state of our knowledge of the archaeological history of Palestine, and the weaknesses in past methods. The pause in excavation there, caused by the war and the later troubles, has provided leisure for bringing publication up to date and for considering future aims and methods. It is to be hoped that the time is not very distant when active work on still obscure sites and problems can be resumed. It is also to be hoped that it will then be directed by excavators who understand the principles of stratification and can apply them.

## Correspondence

Dear Sir,

At the end of the review of *Cambridge Excavations in Minorca* in your September number, it is stated that the cost of the three expeditions was borne by this Museum thanks to the very generous contributions of its Director. I want to make it clear that the Director referred to was Mr Louis Clarke who was Curator at that time.

Yours faithfully,

G. H. S. BUSHNELL,  
*Curator, University Museum of  
Archaeology and Ethnology, Cambridge.*



## Important New Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review*

- PROCEEDINGS OF THE PREHISTORIC SOCIETY, New Series, Vol. xv (1950): 204 pages. London agents: H. K. Lewis & Co., 136 Gower St., w.c. 1. [The articles cover a wide range: the Sudan stone age, British mesolithic to Iron Age, neolithic house-types in temperate Europe. Every article is suitable for inclusion in this section; Dr Clark's first report on the mesolithic site he is digging at Star Carr is of special importance].
- SHAFTESBURY, DORSET: THE STREETS, ROADS AND LANES, by E. JERVOISE. [The first publication of a new society; a praiseworthy first attempt to restore its medieval plan].
- ARCHAEOLOGY AND THE AFTER-LIFE IN PAGAN AND CHRISTIAN IMAGERY, by I. A. RICHMOND. 57 pages, 9 plates. Cumberlege, 1950.
- NEW STUDIES ON RHODESIAN MAN, by J. DESMOND CLARK and others. *Journ. R. Anthr. Inst.*, LXXVII, 1-32. [A re-examination of the Broken Hill discoveries].
- ARCHAEOLOGIA GEOGRAPHICA: published by the *Hamburgisches Museum für Völkerkunde und Vorgeschichte*, Binderstrasse 14, Hamburg 13.
- APERÇU de la structure agraire du Pays de Galles [Wales] occidentale: la structure agraire du Devon et du Cornwall et les enclosures des XIII et XIV siècles: par PIERRE FLATRÈS. *Annales de Bretagne*, LVI (1), 1949. Rennes.
- LES EMAUX CAROLINGIENS DE LA CHASSE DE ST. MARC À HUY SUR MEUSE, ed. MARION, 1948. 450 fr. belg.
- CARTOGRAPHIE PRÉHISTORIQUE, par L. R. NOUGIER. *Bull. Soc. Préh. de France*, XLVII, 1950, 154.
- GEOFFREY OF MONMOUTH AND THE LATIN CHRONICLERS (1300-1500), by LAURA KEELER. *Univ. of California Publications*, Vol. 17.
- DIE ADLERFIBEL VON 1936, und andere Fälschungen aus einer MÜNCHENER GOLDSCHMIEDEWERKSTATT. *Germania*, XXVIII, 1944-50, 54 ff. [Describes fakes of costly Merovingian jewellery made for Nazis and museums under Nazi rule].
- SOME ENGLISH PARALLELS TO THE ANGLO-SAXON POTTERY OF HOLLAND AND BELGIUM IN THE MIGRATION PERIOD, by J. N. L. MYRES. *L'Antiquité Classique* (Brussels) XVII, 1948, 453-72.

## Notes and News

### HOW TO USE OLD MAPS

The following notes are written down at the suggestion of a colleague to whom I had been telling my recent experiences. To my objection that fault-finding was rather thankless business and apt to lead to controversy he replied that criticism was good for people and that it didn't matter what they felt or did about it. I was inclined to agree.

Many old maps of all kinds, from cadastral maps of a parish to world maps, have been reproduced in facsimile by a photographic process, and there is generally an accompanying text. The facsimiles are nearly always so much reduced that the names are illegible. It would be easy to print a list of the names, arranged in some order that can be followed on the facsimile, but this is very seldom done. What purpose, then, does the facsimile fulfil? Beyond showing the extent of a coast line (if any) known at the time—none. The student wants to know the NAMES. Such a list would be far more valuable than the descriptive matter of the text usually is. A better course would be to reproduce a clearly legible facsimile—if necessary in sections. The only use of a facsimile is to save one the trouble of going to see the original (which is often impossible, especially nowadays). Some of the facsimiles in De la Roncière's 'Découverte de l'Afrique au Moyen Age' are wholly illegible, and the originals in Italy. Mannoni's facsimile of Egyptus Novelo is good and legible except where the names are concealed by the binding. It would have taken only a few minutes to copy these from the original (on which they can quite easily be read) and print them in the text. This was not done, and I had to go to Paris to do it myself. Such imperfect publication is unscholarly, and if not useless at any rate falls short of completeness. Ten minutes work on the original would have made all the difference.

The same criticisms apply to the scale when the original map has one. The figures are often illegible on the facsimile, nor is the unit always certain. A simple statement in the text would remedy this.

Students also want to know where they can consult the best detailed account of the map. Such accounts are often open to all the above criticisms, but they may be a little better than those given in books of a general character, such as histories of geographical discovery. Such bibliographical information must be known by the author and should be given.

Students who cannot read facsimiles have to travel to see the originals. They should be told where these are, and not only in what town but in what institution in that town. One may not be able to go there, but one can at least write and ask for a photostat, and 'Rome' and 'Venice' are insufficient addresses. If one goes, one has to write first to be sure that the institution will be open or the map still there. It is also unscholarly to give the name of the institution without the town in which it is; it may be considered to be of world-wide repute but one may not happen to have heard of it.

In attempting to identify the places named on an old map it is often assumed that its author was careless or inaccurate. He may have been, but he probably made the best use of the sources at his disposal, such as itineraries of actual journeys or voyages. The order in which these appear, on a coast-line or along a river, may be correct, even when the actual distances apart and positions are wrong. If a place X, which is unknown, is put between two known places, A and B, it is to be looked for, and may often be found,

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between these places on the modern map. Even if not, it is more likely to be there or thereabouts than 300 miles away on the other side of a range of mountains, where there is another place whose name begins with the same letter, or has some superficial resemblance to it.

In old maps there is a tendency for the same name, sometimes with variable spellings, to recur on a long sequence of maps. A name that cannot be identified on one map, either by the last mentioned method or otherwise, be identifiable on another. Sometimes the latest spelling provides a link with the modern form.

The course of roads may often be determined with precision by noting the places which lie (1) on it (2) on each side of it. By using this method it was possible to plot on the modern map the course of Ogilby's roads (1672) which, being strips or traverses, do not show much of the country through which they pass. But he rarely if ever placed a village on the wrong side of the road.

Places may often be identified by their *relative* distances apart, even when their *absolute* distances (as shown by the scale) are wrong. A place X may be twice as far from A as from B and be found to agree with the position of a known place. It is usually better to try several distances between X and known places before deciding where to place it, and to use proportionate distances, not actual ones.

In setting out to study an old map the first thing to do is of course to locate the places named on a modern map. It is best to begin by making a copy of the original (or facsimile), eliminating any irrelevant matter and drawing it with north at the top. For expository purposes the published account should consist of (1) a legible facsimile (if one does not already exist) (2) a copy of the original drawn as above (3) an outline taken from the modern map with the names on the old map shown (when possible) in their correct geographical positions. It may often not be possible to reproduce a facsimile, though this should always be done when one sets out to produce the definitive account of a particular map rather than the general study of a region. The text should give the reasons for the identifications, and (in the latter case) a list of all names on the original.

The proper elucidation of an old map is a fascinating task, but it involves a lot of hard work, and the drawing of diagrams. It should be remembered that the names have rarely if ever been *invented*; they may be strange and unrecognisable, but they are probably based ultimately on a real place-name, and it is the business of the student to trace its history and identify it. Without such an attempt the text is apt to degenerate into mere useless verbiage, describing what can (with good luck) be seen on the facsimile. Such texts resemble the old-fashioned museum labels which tell one what one can see for oneself and the name of the donor, but omit the provenance. There are many such texts, and it is in the hope of reducing their number in the future that I have written this note.

O.G.S.C.

## MAPS OF THE A-E. SUDAN

In our June Editorial Notes we criticized the Sudan Government maps and suggested that 'air survey might have had priority over some other forms of expenditure'. We have received a letter from the Sudan Government Survey Department, written to 'refute some of this implied criticism'. The writer admits that the basic topographical map of the country (1 : 250,000) is often inaccurate and out of date, especially in desert areas, but points out that the land alongside the Nile as far south as Lat. 13°N. is covered by cadastral plans on scales ranging from 1 : 2500 to 1 : 4000. The demand is small, but is met by sun-prints. The main purpose served is administrative, and though



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‘they do not always include much topographical detail, they do show any islands which are permanent and large enough to have been settled’. He goes on to say:—

‘In addition to these cadastral maps maintained by us, the Egyptian Government has started to publish a series of 1 : 5000 contoured maps in the areas of the 2nd and 4th cataracts, which they have surveyed in connection with the proposed reservoir. Only a limited number of copies of the maps are at present available but an extra set was secured specially for the Commissioner for Archaeology.

‘In addition to these large scale maps we have taken some trouble to secure, through the kindness of the United States Government, copies of some 60,000 air photographs which were taken over the Sudan by U.S.A.A.F. during the recent war. These air photographs can be consulted here and copies of individual prints can also be supplied on payment. Both the present and the late Commissioners for Archaeology have made frequent use of these photographs. We have ourselves already mapped from these photographs about 40,000 square miles; but it will naturally take a long time to map completely the half million or so square miles of country which they cover. We have also just recently obtained an Eagle ix air survey camera and hope to begin using it this winter in one of the Dove aircraft maintained by the Government air service. This camera was not obtained only for survey work and it will also be available for use by other departments, provided they can pay for chartering the aircraft. Most of these air survey developments were described in a recent article in *Sudan Notes and Records* (Vol. xxx, Part 1).

‘I hope that these remarks will remove from your readers’ minds the impression which your note must have created. The Sudan is as much larger than England as its resources are smaller, and one cannot expect accurate maps to cover it completely. Nevertheless, in areas that justify it economically, the position is far from being as bad as your note suggests’.

### ROMAN LIBYA

The following report is reprinted from *The Times* of 28 September, 1950, by kind permission of the Editor.

A British expedition has just completed a two-month programme of archaeological reconnaissance and survey in the Syrtic and Cyrenaican regions of Libya, with the object of collecting information for a proposed map of Roman Libya. The expedition assembled in Tripoli early in July, and moved eastward along the coast of the Greater Syrtis to Benghazi, arriving there after having visited and surveyed many ancient sites on the route.

During August reconnaissances were carried out in the Gebel area of Cyrenaica and in the pre-desert zone to the south, with the assistance of the British military authorities in the territory. A two-week exercise ‘Roman Swan’, organized by Headquarters, Cyrenaica District, enabled the archaeologists to visit many sites not easily accessible. Air survey and photography were made possible by the collaboration of the Royal Air Force.

The British Administration of Tripolitania and the Government of Cyrenaica both gave the greatest encouragement and help to the expedition, and the observations made should assist the compilation of an inventory of those major ancient monuments for the preservation of which the future Libyan State will be responsible.

The areas visited include the region of Sirte (Tripolitania), rich in concrete-built farmhouses of the Roman period, the Mersa Brega zone, with the ancient city of Boreium,

and the forts which defended southwestern Cyrenaica from the tribes of the Syrtica, the Barce and Beda areas of the Cyrenaican Gebel, and the desert route between El Abiar and Mechili. Many ancient buildings were planned, most of them being fortified homesteads of a type already well known in Tripolitania. The sloping reinforcement walls which surround these buildings testify to the earth tremors which shook eastern Libya during the later Roman period and caused the collapse of the great Doric temples of Cyrene.

The work of the expedition has thrown some new light on the Roman roads of Cyrenaica, and on the defensive system organized in the later Roman period. The Roman frontier organization seems to have been less deep than in Tripolitania, and was strongest in the south-western zone towards the Syrtica, from which direction the main barbarian attacks seem to have come. At Zaviet Msus, on the line of the desert route used so frequently during the recent war, a small Roman outpost was covered with the names of soldiers stationed there to protect the important cisterns and control the caravan routes.

Remains of the early Islamic period were also encountered by the expedition, including the site of the city of Sort (Medinet Sultan, near Sirte), and a line of mud-brick forts along the caravan route between El Abiar and Mechili. The last-named were probably intended to protect the important desert track from Egypt to Tunisia, which passed by Mechili, Zaviet Msus, and Agedabia.

The expedition has been working under the auspices of the Map of Roman Libya Committee, which was set up last year under the chairmanship of Professor R. E. M. Wheeler. Generous contributions towards the cost were made by the Royal Geographical Society, the University of Durham, the British Academy, the Society of Antiquaries, the Oxford and Cambridge Craven Committees, and other bodies. Graduates and undergraduates of Oxford, Cambridge, Durham, and Reading universities took part in the field work under the direction of Mr R. G. Goodchild.

The results show that much remains to be done in the recording of ancient buildings in Libya, but that the geographical limits of ancient occupation are fairly clearly defined. A complete archaeological survey would require many years of field-work, but useful topographical information can be obtained in a few seasons of long-range reconnaissance. During its two-month survey this summer the expedition covered nearly 1,500 miles of terrain.

## THE CITY WALL OF KANO

The climate and early type of civilization developed in Nigeria did not aid the preservation of ancient monuments, but arid conditions as found in the Northern Provinces, which approximate closely to those of Upper Egypt, have dealt more kindly with the early efforts of Man than have the damp, humid jungle conditions found over the rest of the country. The only important buildings of antiquity in Nigeria which can be immediately referred back to early civilizations are the ancient walls round the large cities of the Hausa States in the Northern Provinces. Of these, the wall surrounding Kano is probably the oldest and certainly the most important one. The gaps in its history are wide indeed, but one can obtain quite a fair picture of its origins and development from contemporary writings, although other records from the area have not been preserved.

According to tradition, Hausa peoples approximately 1000 years ago migrated from Daura (N. of Kano) under their leader Kano and founded the town bearing his name. It is assumed that the wall was built at the same time. Being an important trading and transport centre, which it still is to this day, Kano was always a prize for conquering tribes and nations, consequently its wall would be preserved and kept in good repair

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for military reasons by all conquerors. The city was kept as a vassal under varying rulers for centuries, until in 1805 it came under the Fulani Empire and was the capital of Kano Emirate.

Although there were many rumours concerning a large and important walled city of Kano, it was not until 1824 that Clapperton actually visited it: the first time a European had entered its gates. He gives a brief description of the wall as being oval in shape, made of clay, 30 feet high, and approximately 15 miles in circumference. A dry ditch ran round the wall, both inside and outside, thus increasing its military value; 15 gates, including one recently built up, gave entrance to the city. The entrances were opened and closed daily and could be defended in case of attack. The next visitor to Kano was Dr H. Barth in 1850, who found traces of an older, dilapidated wall inside the city itself. This wall was not complete but may well have been the original site of the early township before it expanded to its present size. From that time, Kano was no longer a city of mystery but open to free trade with other Europeans.

During the British conquest of Nigeria a punitive column of British troops in 1903 attacked the main gates of Kano, but the attack failed. A neighbouring gate (Nassarawa gate) was breached; the town was stormed with few casualties; the Emir's palace was occupied and order quickly restored. Kano then came under the administration of the British Government.

The present state of the wall is bad. No repairs seem to have been undertaken since the British conquest of the city except that certain entrances have been widened to allow the entry of motor traffic into the city proper. In parts the wall merely consists of a high mound of sun-hardened mud and the protective ditches are gradually filling in to ground level. The gates and platforms built for defence have been removed, and many entrances hollowed out to form leper-holes. There are still 15 gateways into the city. The old ones are too small to allow even a small car to drive through in comfort and the wall in their neighbourhood is usually in a better state of preservation than in the neighbourhood of the new entrances to the business quarters. All remains of the ancient wall noticed by Barth have completely disappeared.

Unless preservation is undertaken quickly one more irreplaceable link with early historical times will have been irreparably damaged.

P. B. ADAMSON.



## Reviews

JEBEL MOYA. By FRANK ADDISON, F.S.A. *The Wellcome Excavations in the Sudan, Vols. I (text) and II (plates), published for the Trustees by the Oxford University Press, 1949. Price £6 6s.*

It seems that the gestation period of the greater excavators, like that of mammals, increases in proportion to the size of their excavations. Dr Reisner, who dug continuously for three decades, died before his first substantive publication appeared; and Sir Henry Wellcome's excavations have had to wait for nearly forty years. Though covering four seasons only (1910-14) they were conducted on a colossal scale. The main purpose was philanthropic; but as a business man well versed in scientific procedure, Sir Henry took care to have the work properly supervised, his archaeological adviser being Dr Reisner, whose method was adopted and some of whose trained Egyptians were employed. The site selected was a poor hill-village in a granite *jebel* in the Gezira, 20 miles west of Sennar on the Blue Nile where now is the Mekwar dam. It was not only a village but also a huge graveyard; the huts, probably just grass *tukuls*, had left no trace behind, and the evidence consisted chiefly of what came from a few of the graves and the miscellaneous debris of the inhabitants. Thanks to the scale of operations so much was found that it is possible, for once, to trust negative evidence; from which the first occupation appears to have begun not before 1000 B.C. and to have ended about 400 B.C. The culture was neolithic, with a few stray imports from the north Sudanese kingdom of Napata. The absence of Meroitic imports suggests that Jebel Moya was abandoned before the capital shifted southwards from Napata to Meroe near Shendi.

Sir Henry's selection of Jebel Moya was probably not primarily based on its archaeological importance (which was not much) but on other grounds. He was a benevolent autocrat, and liked to sit in his tent and direct the operations of his staff like a general. For security reasons the expedition's camp was run on military lines, complete with a Camp Commandant, armed guards and night watchmen perched on look-outs (and usually asleep). A megalithic building called the House of Boulders was built where he might reside. 'Uplift' was the order of the day; efforts were made to encourage thrift and sobriety amongst the long-suffering villagers, and an Order of the Peacock instituted for teetotallers. (The peacocks were imported from England and kept in the camp zoo). All who applied for work were taken on, and it was up to the staff to find work for them. As may be imagined, the archaeological work suffered, for the archaeologists (never more than three in number) were outstripped by the pace of the excavations and their records necessarily got behindhand. Nor were they allowed free hand. Their weekly diaries were submitted to the Chief, who returned them with criticisms. They were also bound by a very severe written contract.

Nevertheless, even if the results were disproportionate to the total expenditure—which cannot have been far short of £100,000 (of pre-1914 value)—when the final balance is drawn there is beyond all doubt a substantial profit to be recorded. This book is the chief item. It has been compiled by an experienced archaeologist who knows the Sudan but took no part in the excavations himself. Writing, as he says, as a detached observer, he has succeeded in producing a lucid and even readable account of the work. That is

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a really remarkable achievement, for in spite of Sir Henry's insistence on method—perhaps even because of his over-insistence on it—the materials with which Mr Addison had to deal were often imperfect and sometimes contradictory. Within those four years there were constant changes of method and of archaeologists, resulting in confusion. The finds passed through many vicissitudes; some have disappeared, including some of the best; labels were obliterated or altered. But so huge was the scale of operations that, even so, much was left to work on.

It must not be forgotten that when Sir Henry began to dig in 1900 modern methods of excavation had only just begun to be used in Africa and Asia, and that he adopted them. He was not a Pitt-Rivers (though there are some points of resemblance) nor could he even have claimed to be an archaeologist perhaps; but he had the root of the matter in him; and when he decided to excavate he turned for advice to the man who did for Egypt what Pitt-Rivers did for Britain, namely, Dr Reisner. This admirable book, to be followed (soon we hope) by another, is a fitting memorial, and his Trustees, in publishing it, have earned the gratitude of all archaeologists. O.G.S.C.

THE SARAWAK MUSEUM JOURNAL, issued by the Museum, Kuching, Sarawak; May, 1949 (*vol. 5: NS. no. 1, OS. no. 16*). *Annual Subscription, covering at least one issue yearly. \$6.00 (Straits).*

It is always a good sign when a publication tells one all about itself, how to get it and such like, as this one does. The fact that this one is edited by Mr Tom Harrison is a guarantee of vitality. The contents are a mixture of anthropology, archaeology and natural history; several of the articles are by Borneans, one in a Bornean language. Space forbids lengthy analysis; the editor's introduction strikes the right note, and we wish him every success in his work of introducing the techniques of western science to the east. (Could not the making of good half-tone blocks be added to them? There seems to be only one firm that can do so between Europe and the Pacific—and it did not make those in this Journal). An article by a Malay scholar on the art of Anyi, a Kelabit, reveals a live art and an artist who, besides the normal mediums, can work in others less durable. His formal patterns are most pleasing. Could he not be asked to design a new cover for the journal, in collaboration with the editor? There is a most interesting account, by Mr Jamuh, a Dyak, of the erection of large wooden poles which will be of interest to British prehistorians, who would as gladly read a description of the construction of Woodhenge by a Bronze Age Briton. There is much about languages, a remarkable tale of a snake that was seen to glide through the air for 250 feet, an account of recent Sarawak stamps, and notes on orchids. There is a judicious admixture of readability and specialism. The journal will be found valuable by all (including specialists) interested in both man and nature in the great island of Borneo; we are sure that it will prosper under its present editor and wish it every success. Both it and the subjects dealt with fully deserve official support. One minor point: in view of its bearing on human history, could not geology find a place in it? O.G.S.C.

THE OLD STONE AGE IN THE ANGLO-EGYPTIAN SUDAN. *Sudan Antiquities Service, Occasional Papers No. 1. Deighton Bell, Cambridge, 1949.*

Northwest of Khartoum, 1 kilometre below the junction of the Blue and White Niles, the Nile has on its left bank a deeply-cut torrent-bed called Khor Abu Anga, which widens out suddenly at about a kilometre from Omdurman, *i.e.* at two or three kilometres

from the Nile. The bed studied by the author is situated, if I understand aright, immediately above this town, whose inhabitants exploit its deposits for the materials of their houses *en pisé*.

Resting upon (1) the Nubian sandstone series, with ferricrete layers and mudstone *passim*, one can distinguish the following strata : (2) decomposed Nubian sandstone, with bands of ironstone pebbles in place : (3) occasionally, iron-cemented conglomerate (max. thickness 5 feet) : (4) coarse gravel of ferricrete sandstone and indurated mudstone, sometimes as much as 3 feet thick : (5) fine ironstone gravel, often cemented with clay (average thickness one to two feet) : (6) in places, especially upstream of resistant barriers of layers 1 or 3, a layer of white calcareous sandy clay (with shells of *Limicolaria flammata*) probably deposited at the same time as the 'mesolithic' of Khartoum (1).

There have been found *in situ* 185 worked stones, almost all of them artifacts of the 'Pre-Chellean' and 'Chellean' type (44 in layer 4 and 4 at the base of the next layer) ; 'Early Acheulean' (15 in layer 4 and 6 in the deposits of layer 5 immediately above) ; Upper Acheulean (1 in layer 4, 21 in the next above it, where were also found 31 smaller hand-axes assigned to the 'Late Acheulean'). Lastly, from layer 6 have come a dozen hand-axes, one of which is thin, with carefully chipped base, belonging to a developed type ; an ovate with inverse flaking of slight extent and a 'rough ferricrete lancehead' : a thick foliated hand-axe which the author regards as suggesting the presence of the Toumbian ('Sangoen'), various implements typical of which have been found on the surface or in gravels relaid at an epoch 'where the topography was beginning to assume an appearance similar to that of today'. This is in my opinion a rash hypothesis ; one could find similar specimens in any Acheulean deposit. With these surface or rearranged objects are found discoid cores (or those of the transverse type of Victoria West) and their flakes.

More than 90 per cent of these implements are of 'ferricrete sandstone', the others being of 'silcrete sandstone', except for some of 'indurated mudstone', rhyolite, basalt or quartz.

Is the Toumbian then derived from the 'Late Acheulean', or is it independent thereof ? Are certain foliate specimens the prototypes of the 'Solutrian-like artifacts' of the Egyptian predynastic period ? Such are the questions which the author asks before proceeding to the examination of other sites to the south and as far to the north as Wadi Halfa and Faras. We may mention the site of Wadi Afu, 80 kilometres south of Khartoum, on the left bank of the White Nile, where a thin deposit of coarse gravel has yielded objects of the Upper Acheulean. Those found elsewhere, often *in situ*, are assigned to the 'Prechelleo-Acheulean' (pebble-tools collected from gravel at 52 metres on the top of Jebel Nuri near the 4th cataract) ; and to the 'Chelleo-Acheulean', with flakes of the 'block-on-block' technique (Clactonian) from the 10-metre terrace of the Atbara above Sarsareib. A kind of 'Epi-Levalloisian' with typical cores (Pl. 27, fig. 1), such as occur in the Aterian, have been found superficially in the same region. At Amara between the 2nd and 3rd cataracts, another surface site has yielded a complex series of implements amongst which one observes planes related to those of the Fayum Neolithic B. Other implements have been found far from the Nile (wells of Abu Tabari, 400 km. w. of the 6th cataract), which suggests a much damper climate than that of today. That was true also, as we know, of the period of the 'Mesolithic' of Khartoum.

The discoveries at Abu Anga are important. They are a satisfactory sequel to that of Khartoum, and Mr A. J. Arkell, by these two publications, has notably advanced our

<sup>1</sup> See review in *ANTIQUITY*, September 1950, 151-4.



knowledge of the prehistory of the eastern Sudan. The illustrations are fully adequate, but scientific publications should refrain from publishing figures without descriptive titles, enforcing a laborious search in the text and making the reading of it unnecessarily difficult.

R. VAUFREY.

THE EARLY CHRISTIAN MONUMENTS OF WALES. By V. E. NASH-WILLIAMS. pp. xxiv+258, with 71 plates and 261 illustrations in text. Cardiff: University of Wales Press, 1950. £4 4s.

Early Welsh history from the end of Roman rule to the extinction of native independence in the Edwardian wars is a story that must be reconstructed from many different sources. Archaeology has a large contribution to make, but many of the antiquities which should illuminate this study have yet to be identified. One series is known and covers the whole period. The early Christian crosses and inscriptions start in the 5th century and continue, as this work shews, in an unbroken succession. A comprehensive inventory and survey has long been wanted. Dr Nash-Williams has now supplied this need and all students in this and allied fields will be grateful for the industry and scholarship, which are apparent on every page of this book.

The major part of the volume consists of an inventory, arranged alphabetically under counties and comprising 415 monuments. The descriptions are clear and concise and the comments, iconographical and historical, are factual and authoritative. The selection of references is not always consistent. Normally only those publications which make a substantive contribution to the study of the monument are cited. A rather fuller bibliography would in many cases have been convenient but the student can always refer to the author's earlier list in the *Bulletin of the Board of Celtic Studies* (vol. VIII, 1936) for a reference to the sources in which the early vicissitudes of the monuments and similar questions may be examined.

Very few omissions and corrections need to be noted. Removal for their better preservation is a continual process and it is too much to hope that any list will be fully up to date in its record of the present position of these monuments. The crosses from Erwhelm (no. 47) and Neuadd Siarman (no. 65) are now in Brecon Museum; the inscribed stone from Llanol (no. 6) is in Bangor Museum. More important, the inscribed stone from Llanfaelog (no. 10) has been found in use as a lintel in the barn at Pensieri bach. Incidentally this inscription can now be seen to read MAILISI with a horizontal final i as shewn in the original drawing. It might be added that both Eliseg's Pillar (no. 182) and the Maen Achwyffan (no. 190) are now under the guardianship of the Ministry of Works, a destination which might well be considered in the case of other important monuments which it is desirable to preserve *in situ*. Under no. 183 for Holyhead railway read *road*; under no. 390 for St. Dogmaels read *St. Dogwells*. The following monuments should be added to the inventory:—Caernarvonshire: the font at Pistyll with debased plaitwork and the cross-decorated boulder from Glan Beuno on the Seiont, now in the Eglwys y Bedd at Clynnogfawr. Pembrokeshire: the rock-cut cross on the cliff west of the church at Nevern. The boulder now at Clynnogfawr has a carefully cut cross. The tradition connecting it with St. Beuno is probably trustworthy and it thus provides a welcome confirmation of the early date proposed for the oldest stones in group II. Finally it should be noted that in the case of group IV (Transitional Romanesque Monuments, 11th–13th century) the inventory is less comprehensive than in the earlier periods. To mention only Meirionydd: there is no record of the inscriptions at Llandecwyn and Llanelltyd, which certainly fall within this classification.

Dr Nash-Williams' introduction arranges the monuments in four groups: simple inscribed stones (5th-7th century), cross-decorated stones (7th-9th century), sculptured crosses and cross-slabs (9th-11th century) and transitional Romanesque monuments (11th-13th century). The second group continued in use locally beyond the end of the period indicated and the author lists some 20 examples which he considers to be survivals. This list should probably be extended. In particular the more elaborately designed crosses (e.g. fig. 5, nos. 9, 29 and 36) seem likely to be contemporary with group III.

There are also valuable appendices analyzing the letter-forms and the decorative patterns and representational motifs. The patterns, both in this appendix and in the text, are described by reference to Romilly Allen's well known classification. The long and excellent bibliography includes Macalister *Corpus Inscriptionum Insularum Celticarum* without reference to the second volume (1949), which presumably appeared too late to be used. The book is illustrated with an admirable and carefully selected series of photographs and drawings. Many of the 71 plates shew as many as 6 or 8 of the simpler, smaller monuments. Some, like the writer, will regret that so many of the inscriptions have the letters outlined in chalk. This practice facilitates the quick reading of the lettering, but aesthetically it detracts from the appearance of the monument and scientifically it makes the checking of a doubtful letter more difficult. This is a minor point and this notice must not end on a note of criticism. Dr Nash-Williams has carried out a magnificent piece of work and the University of Wales Press has attained the highest standards of production.

C.A.R.R.

CULTURE IN EARLY ANGLO-SAXON ENGLAND. By D. ELIZABETH MARTIN-CLARKE. *Baltimore, Johns Hopkins University Press, 1947. (London: Geoffrey Cumberlege). pp. 100, pl. XXVIII. Price 12s 6d net.*

Mrs Martin-Clarke has long taught closer collaboration between literature and archaeology in the field of Anglo-Saxon Studies. This attractively produced book (published in America, and being a set of lectures delivered by the author at the Johns Hopkins University in 1945) is the printed expression of this creed. Mrs Martin-Clarke uses the archaeology of the period as a general framework to what she writes. We read in the preface that 'the subject-matter is for the general public interested in early culture'. In fact the book forms a handy introduction to aspects of early Saxon literature, and brings home successfully its general relationship with the archaeological material. The literary matter will be of interest to students of archaeology not familiar with the literary background, and *vice-versa*. On the archaeological side however, which predominates, the book does not advance far beyond Stjerna and Baldwin Brown. The only new matter that obtrudes itself is the Sutton Hoo material, which is continually referred to. In 1945 the author was still necessarily dependent almost entirely on the 1940 accounts of the find, but the fact remains that a great deal of Mrs Martin-Clarke's Sutton Hoo material has inevitably been superseded, and some of it is unsound. Mrs Martin-Clarke does well to emphasize the visual approach to early literature. 'Archaeology forces the student to use his eyes'. The visualization of objects 'makes the students more alert to the symbolism which is the basis of all language'. It is all the more unfortunate that the selection of illustrations and the use made of them does not live up to this promise. The interplay between text and pictures is not as happy or illuminating as it could have been, nor has enough care gone into the correlation of the two. To quote only two rather obvious instances, there would have been no justification even as far back as 1940 for the version of the Sutton Hoo purse that appears as Plate XXII, a pathetic illustration of the corresponding text (p. 90).

And why is not Plate XVIII (captioned 'Viking Camp at Trelleborg'), explained somewhere for the uninitiated for whom the book is meant? To the archaeological eye not familiar with the details of the unique Trelleborg plan it looks like a close-up of patches on the bottom of a bronze bowl that has somehow attracted the wrong caption. To the literary student and general reader it will probably be meaningless. Perhaps some of these short-comings in the production of the book should be put down to the difficulties of transatlantic liaison between author and publisher, but they are none the less unfortunate. In general, there is a lack of precision in wording and a looseness in description of archaeological material that is apt to be misleading or difficult to grasp. The general level of accuracy seems disappointingly low, as far as archaeology is concerned.

R. L. S. BRUCE-MITFORD.

# STUDIES IN ANCIENT GREEK SOCIETY, THE PREHISTORIC AEGEAN.

By GEORGE THOMSON. *Lawrence and Wishart Ltd., 81 Chancery Lane, London, W.C. 2, 1949. pp. 624 with 85 figures and 12 maps. 42s.*

This volume is planned as the first of a series of studies in ancient Greek society and is devoted to a consideration of the prehistoric Aegean. Our knowledge of the prehistoric Aegean depends largely upon archaeology. Any scholar, therefore, who has not had the advantage of a first hand acquaintance with the archaeological evidence is rash in attempting such a study as this without expert guidance. The bibliography shows that Professor Thomson has consulted many books, but he seems as far as archaeology is concerned to rely on secondary rather than on primary sources. In a study of this character the original reports of the excavators should always be regarded as the basic documents.

In his seventeen chapters (nearly six hundred pages of text) the author often fails to grasp the implications of the archaeological evidence and sometimes misquotes it. No great palace has been excavated at Boeotian Orchomenos (p. 193). It is hardly correct to say (p. 29) that Mycenaean princes 'did nothing to improve the technique of production'. The whole development of culture in the Late Helladic, or so-called Mycenaean, period shows that there was a steady advance in technique in most crafts, notably in architecture, pottery, and metal working. Schliemann did not discover at Mycenae 'mostly in tombs' (p. 239) a large quantity of female terracotta figurines. Does 'statutory groups' on p. 182 mean groups of statues? In any case it is hard to see how they could confirm the legend that Amazons founded the sanctuary of Artemis at Ephesus. Burial in tholos tombs was not a standard Minoan method of interment and the so-called tholoi of Mesara of the Early Minoan period can never have been tholoi (p. 249). On the same page it is stated that 'one of the sepulchres in the Grave Circle at Mycenae was in continuous use for two centuries (1450-1250 B.C.) and a family likeness has been recognized in the skeletons'. The skeletal material of the Grave Circle at Mycenae, which is very slender, has never been scientifically examined and the graves themselves date, as the author himself says on p. 371, from the 16th century B.C.

The author has studied Greek legend and mythology profoundly and has also read much comparative anthropology, but his results are most speculative and cannot be regarded as proved. He mixes archaeology and legend, as in his suggestion that the neolithic users of Dimeni ware in Thessaly were Lapiths and introduced Greek into prehistoric Greece. From this he further deduces that Poseidon at Corinth is a Lapith (pp. 184, 265). On page 383 we read 'Danae's subterranean prison seems to be a faint memory of the Shaft Graves, confused with the custom of secluding girls at puberty'.



With many of the author's points such as the evidences for matriarchy and for the pre-historic origins of classical Greek religion and belief no one can quarrel and he often refers to anthropological parallels with good effect. His method of argument, however, is unsafe, for he is apt to confuse the factual and hypothetical and he treats all legends and myths as if they were of equal value. Indeed by using such methods an ingenious writer could prove almost anything. The fifth section of the book, on Homer, is the best because here his essential scholarship and knowledge of ancient literature have full play unencumbered by mythological speculations and archaeological misunderstandings. The Homeric student will find in it much sound sense and pointed criticism as well as a genuine feeling for the beauties of Greek literature. A.J.B.W.

LOUGH GUR EXCAVATIONS. 'Three Marshland Habitation Sites', by S. P. O'RIORDAIN; 'Two Barrows at Ballingoola', by MAIRE MACDERMOTT (*J.R.S.A.I.*, 1949, 126-145—not sold separately). 'Carraig Aille and the "Spectacles"', by S. P. O'RIORDAIN (*P.R.I.A.*, LII, c, pp. 3-15s).

These reports are an addition to the already substantial record of Prof. O'Riordain's work at Loch Gur and to our knowledge of house types. The structures recorded are three houses or huts (Grange and Ballingoola III and IV), the first at least dated not earlier than the Iron Age; two ring-barrows (Ballingoola I and II) of the Rathjordan type, perhaps not much later than a scatter of neolithic and beaker sherds found beneath them; two stone forts (Carraig Aille I and II) with associated buildings, dated to the 8th to 11th centuries A.D.; and the similarly dated undefended habitation site of the Spectacles.

With such wealth of material selection is inevitable and attention will be directed here to the house types and the enclosures containing them. Commencing with the humbler marsh sites, Grange is shown to be a small enclosure with earth bank and ditch containing a round house 5 m. in diameter, placed to one side of it. The house was floored with clay, walled with wattle and clay and provided with a central hearth and another hearth outside the door, while the clay floor extended outside the house and served as a working area. Some timber was laid down inside the entrance to the enclosure where the animals churned up the turf. This is a typical peasant homestead; the house was no doubt used mainly for sleeping and storage and, as Prof. O'Riordain wisely comments, life was lived largely out of doors.

Ballingoola III and IV were two contiguous wattle and clay huts some 6 m. in diameter without any surrounding enclosure. Prof. O'Riordain believes that the walls were banked externally with clay and draws a comparison with Dr Bersu's externally-banked farmyard wall at Ballacagan in Man, but such simple structures could arise independently. Their most interesting features are their shallow ditches immediately outside the line of the wall. Such have been found by Mr Stevenson in Scotland and it would be useful to know how far these are mere quarry ditches and how far they served as 'dry areas' to carry off the rainwater from the roof; calculation might show whether the excavated material was in excess of the quantity required for the former purpose. Prof. O'Riordain suggests use of the two huts as herdsmen's shelters, but the finds hardly support even this degree of habitation and, while one has an unenclosed central hearth three times remade, there are no external hearths or working places. These facts, and the absence of any enclosure, suggest rather that the buildings were used for storage or animals.

In contrast to these two elementary structures, which might be built in any age, the Dark Age sites were of much more pretentious character and yielded a wealth of occupation material, which will be invaluable for comparison with contemporary sites in Ireland and Western Britain. Of the three, Carraig Aille I and II, perched uncomfortably on a

ridge, seem to be defensive in origin, though houses gradually spread over and beyond the wall of II. Each had a well-faced rubble wall 11 to 14 feet thick and provided with steps to reach the wall-head, a feature which is not necessarily defensive. The maximum wall height is now 3 feet 6 inches but is thought originally to have been 6 to 8 feet, and it might be possible—and would be valuable—to test this by measuring accurately the quantity of debris lying along a 10 foot length of wall. The areas enclosed were small, the average internal diameter not differing much from 100 feet in either fort, and originally the arrangements for shelter within them seem to have been of the most transitory. From this fact, from their site and from the enclosure external to I, which probably served for cattle, they are likely to have served as places of refuge in tribal warfare.

Despite its site, however, Carraig Aille II ceased to be such; a gap was made in its wall and a small but wealthy suburb grew up outside. Here, and in the undefended Spectacles site, more or less round buildings occur, but the tendency was to the rectangular. An interesting type was a walled rectangular courtyard covered over at one end; the house portion being divided from the yard by a wooden partition. Wood was extensively used in construction. The narrow walls of faced rubble would have stood to little height unless supported internally and externally by a wooden framing which is revealed by postholes cut in the rock. Sometimes earth replaced stone as a filling. Prof. O'Riordain has here identified a type of building, related to that found by Dr Bersu in Carnarvonshire and still surviving in the Faeroes, which is in essence a timber-framed one. It is to be suspected that many of the slight walls surviving as a ruin of stone or an earth bank in our upland areas were similarly timber-framed, and are to be contrasted with the thick-walled blackhouse of the Hebrides, where wood could be found only for rafters.

Prof. O'Riordain is to be congratulated on recovering so much of these ruined structures. Working with student labour, and by the method of excavating in rectangles divided by baulks (subsequently removed) which served as barrow-ways, something was necessarily sacrificed in the highly complex stratification of uneven rock floors, and the classification of the great range of articles of adornment and domestic use is based mainly on comparison with already known material. The finds are excellently illustrated with line drawings, and a few less successful photographs, and the student of the Dark Ages in the West is greatly indebted to Prof. O'Riordain for the heavy work entailed in their examination and description. L.S.

THE WILBOUR PAPYRUS. By ALAN H. GARDINER. I. *Plates* (1941), 73 pls. in fol. II. *Commentary* (1948), 216 pp. with 3 tables, 2 maps. III. *Translation* (1948), 138 pp. Oxford University Press.

The Wilbour Papyrus in possession of the Brooklyn Mus., U.S.A., belongs with a length of 10 m. to the most voluminous Egyptian manuscripts. Written in a very cursive Hieratic, its main part contains an account of measurements and assessments of land made over a period of about twenty-three days in the fourth year of Ramses V, i.e. about 1150 B.C. and 10 years later than the famous Papyrus Harris I of the British Museum with the report on all the donations of Ramses III to the Egyptian temples. The second part of the Wilbour consists of schedules concerning assessments of special fields of the Crown (khato-land of the Pharaoh) all situated in a Middle Egyptian region of 140 km. in length north of Minieh up to the Fayûm in the north. Apart from Upper Egyptian temples, too, the large Theban temples up to Hermonthis (south of Luxor) appear as land-owners, but no Lower Egyptian temples, apart from Memphis and Heliopolis are mentioned. Tax-lists offer no attractive reading, the more as they are composed in

special terms concerning the position, owner, kind of soil, measures for experts. Their evaluation for the history of administration requires much patience and sagacity. Dr Gardiner has spent many years' work on it: in his *Ancient Egyptian Onomastica* (1947) he dealt with all the geographical schedules of Egypt. In order to clear up the technique of seizing and delivering he collected relevant documents in the 'Ramesside Administrative Documents' (1948) and interpreted them in *JEA.* 27. Having done this the editing of the Wilbour Papyrus became a pioneer's work, which in its learned commentary transgressed by far the scope of an *editio princeps* and will in future form the cornerstone for our knowledge of the agricultural administration in Egypt at a time when the state showed all signs of decay by fasting-strikes, riots and robbery.

As an interesting historical example taken out of the schedules of property I should like to quote the evidence about the real property of a royal harem in the town of Miwêr (Moîris) at the entrance of the Fayûm, a main region for catching fish and birds for court, where already in the XIIIth Dynasty a royal harem existed where Teje, mother of Echnaton, had her widow's seat (Med. Gurob) and where probably the Hittitic wife of Ramses II resided. Apart from explanations on questions concerning topography and cult, which can be illustrated by maps and tables, on metrology and other matters, the main question which the Wilbour Papyrus had not answered was: Did the temples retain the taxes or did they pay them to the Crown? Following a hypothesis of Černý, Gardiner decides for the latter solution, because it even happens that temple-property is administered as khato-land of the Pharaoh! Thus temple-land could be seized under certain circumstances (bad cultivation?). The anomaly contained therein, that the granaries of the residence remained empty in spite of exact assessments, is explained by Gardiner by pointing to the exploitation of the state by the family of the almighty high priest of Amun Ramsesnacht: one of his sons Usimarênacht, as steward of Amun, was not only one of the great cultivators of the crown-land in these districts situated at a great distance from Thebes (the second place is held by a standard-bearer of the Residence), but he was perhaps also the chief tax-master of the whole country.

I should like to point out another feature which the Wilbour Papyrus reveals: the division of the land among innumerable small-holders of all classes who cultivated the soil at a personal charge. Such small-holder properties in combination with a slow, red-tape administration mean a certain loss for the state in the due delivery to the central granaries. With the edition of the Wilbour Papyrus, in respect of printing technique a first-class work of the Oxford University Press, Sir Alan has given a rich present to Egyptology itself just before his 70th anniversary.

HERMANN A. J. KEES.

SEVEN PRIVATE TOMBS AT KURNAH. By N. DE GARIS DAVIES. (Mond Excavations at Thebes II) By ALAN H. GARDINER. x, 59 p., 41 pl. (4 in colours). London: *Egypt Exploration Society*, 1948.

To commemorate the many years' work of the late Sir Robert Mond for the tombs of the Theban necropolis Lady Mond has made possible the publication of this volume. Dr Gardiner has looked over the descriptions left by the late N. de Garis Davies (who died in 1941) and has especially examined the translations of the texts. In the course of the excavations made by Mond most of the seven tombs were uncovered or cleared. They are situated near the Ramose-Tomb (No. 55), published by Davies in 1941, but, in contrast with it, they do not belong to the persons of his rank of the necropolis.

If we except the decoration of the tomb of a steward of the high priest of Amun Meri in the time of Amenophis II (No. 45), partly preserved at a usurpation in Ramesside time, they originate from Ramesside time and do not show any more the richly coloured



vivacity of e.g. the two pre-Ramesside tombs (No. 51,217) formerly published by Davies in his *Two Ramesside Tombs* (1927). Scenes taken from life, e.g. representing the deceased in the society of ladies, in the act of fishing and bird-catching such as are to be found in the tomb of the high priest of the Month of Hermonthis Hatiay (No. 324) are rare. They still recall the composition and motives of the Amarna age. Subjects taken from the sphere of ceremony and cult predominate there: among them the representation of a festival procession of the holy barge of the Month of Hermonthis when visiting the subsidiary temple of El Tôd and the departure of the statue of Thutmosis III, where the tomb-owner was a high priest (in the tomb of Chons called To of the time of Ramses II) (No. 31). Moreover the tombs offer many interesting subjects for the genealogies of the priests of the Ramesside time, e.g., that high priest represents two viziers as his ancestors and the formerly quoted Chons claims the same vizier Usermonth as his relative! All this is a characteristic reaction as to the denying of tradition in the Amarna time. It is with gratitude that we think of the late N. de Garis Davies whose skilled hand preserved for us so much of the charm of the Theban tombs.

HERMANN A. J. KEES.

SIALK, GIYAN, HISSAR AND THE INDO-IRANIAN CONNECTION. By D. H. GORDON. *Man in India*, xxvii (1947), no. 3, 195-241.

In this long and necessarily complex paper, Col. Gordon has made a valiant effort to correlate the stratigraphy and chronology of the three main excavated sites of pre-historic Persia, and to bring them into relationship with those of north-western India. To some extent his survey covers ground partially gone over by Dr Donald McCown and the present writer, while since the publication of the paper, Dr Claude Schaeffer's monumental study of Western Asiatic stratigraphy and chronology has appeared.

Gordon's main contention is the necessity of a short chronology for the Persian material after Early Dynastic times, and in his scheme Hissar III is dated 2100-1100, with its final phase overlapping with Giyan I and Sialk VI (the 'Necropole B'). Now some overlap between the last two periods mentioned seems certain enough. Schaeffer, discussing the matter in great detail and making certain important corrections in the dating of the cylinder seals and scarabs from the B Cemetery, would date the A Cemetery (Sialk v) at 1400-1200, and the B tombs (vi) as 1250-1100 (higher dates by a couple of centuries than those originally proposed by Ghirshman). Schaeffer would conflate Giyan I and II and assign them to a period c. 1550-1200—a considerable reduction on Ghirshman's dates of c. 1800 for the beginning of Giyan II—Gordon would indeed set it back to 2000.

Both McCown and Schaeffer have found it necessary to assume a break between the strata of Giyan v and iv (the Giyan stratigraphy is numbered from top to bottom, contrary to that of Hissar or Sialk). The former would end Giyan v in mid-Uruk times, and begin iv 'not later than Early Dynastic III' (*Comp. Strat. Early Iran*, 48)—say about 2500. The latter, not concerned with the date of Giyan v, which is outside the chronological limits of his study, begins iv/III at 2100. Gordon however would prefer to bring down the end of v to about Early Dynastic I times, and to make no chronological break between v and iv. Although McCown feels there was 'unquestionably a long gap between the end of v and the beginning of iv' (*ibid*, 48), and Schaeffer in his *Table Synoptique IX* indicates such a gap, of at least three centuries, yet in the text he is more moderate, and inclines to 'une rupture stratigraphique [implied by the use of the site as a cemetery] accompagnée probablement d'un hiatus' (*Strat. Comp.*, p. 464). As Gordon says, McCown's case for a long hiatus is not unassailable.

It is worth while remarking, in parenthesis, that such differences of opinion are made possible only by the wholly inadequate recording of stratification in the Tepe Giyan excavations. To those of us trained in European archaeology the normal standards of Oriental excavation and publication come as a severe shock when we turn for information to the published reports. It seems to us astonishing that such a ludicrous travesty of a stratified section such as that given in the Giyan report (Pl. 3) could be published in 1935, and this is unfortunately all too typical of the products of the expeditions sponsored by many authorities in Western Asia. The 'section' of Turang Tepe A published in the report of the American excavations in 1932 is even less adequate than that of Giyan, and of the stratigraphy of Turang Tepe C, Schaeffer ruefully remarks ' nous n'avons pas pu obtenir de coupe ' (*Strat. Comp.* p. 452). It is perhaps easy to criticize—though for evidence that European standards *can* be applied one has only to turn to the volumes of *Ancient India* published since the war. But in general we look in vain for interpretation and record of detailed stratification in the manner of Bersu, van Giffen or even of Pitt-Rivers half-a-century ago.

So far Gordon's contentions do not involve, on the whole, very serious discrepancies from (for instance) Schaeffer's chronology in general terms. But in his dating for the three phases of Hissar III he attempts a scaling-down for the end of the period which is far below any limits hitherto set. McCown in his classic work of 1942 set out to demonstrate a long chronology in which the Hissar III period ran from Early Dynastic I to Akkadian (i.e. about 2800–2300 on the current short chronology of Mesopotamia). I felt the difficulties inherent in this view when I was trying to correlate the Indian material with the Persian in 1943, and suggested making Hissar III ' not earlier than Akkadian and probably some centuries later ' (*ANTIQUITY*, xvii (1943), 181). But by 1947 I saw my chronology was too short, but did not feel ' that a date earlier than c. 2500 for the beginning is really defensible, and would prefer to bring it much nearer 2000 ' (*Ancient India*, No. 4 (1947–48), 32). A discussion with McCown early in 1948 ended in our agreeing on a terminal date for Hissar III of 2000, and the publication of Schaeffer's book later in the year, in which he gave dates of 2300–2000 for Hissar III, gave welcome conformation of our views. Gordon's dates of 2100–1100 do not seem to me to be at all admissible.

The evidence has been admirably marshalled and discussed by Schaeffer. The spiral-headed and animal-headed pins and wands (surely not all unfinished pins as Schaeffer would have them—what about kohl-rods?) link Hissar III with the Caucasus, Anatolia and Luristan; the mid-rib, hook-tanged daggers and spear-heads have good parallels at Ras Shamra, Cyprus, Tarsus and elsewhere, and like the axe-adzes are of ultimately Early Dynastic types, but the associations in the sites mentioned all converge to indicate a date round about 2200–2000. Schaeffer makes a significant grouping of the rich tombs of Maikop, Alaca Hoyuk, Turang Tepe (' The Treasure of Asterabad ') and others, with the Hissar III material in a similar chronological context. Historically this sudden acquisition of wealth and of Sumerian metallurgical techniques by the outer barbarians about the time of the temporary collapse on the death of Naram-sin, and the inroads of the raiders, seems apt enough. The dating of Hissar III as 2300–2000 seems to me to fit in admirably not only with the evidence from the Aegean, Anatolia and the Levant discussed by Schaeffer, but with the Indian evidence as well.

Gordon is brave enough to discuss the Aryan invasion of India in its archaeological context. Like the Druids in Europe, these Indo-European people have become so suspect owing to the nonsense written about them by fools (and, with the Aryans, by knaves as well), that respectable archaeologists hardly dare mention them. But after all, they did

exist, and constitute a problem of first-class importance in the archaeology of Europe and Western Asia. Gordon summarizes the pieces of evidence pointing to movements of peoples eastwards into Baluchistan and India early in the second millennium B.C. (the axe-adze from Mohenjo-daro and other copper tools and weapons) and the Shahi-tump and Jhukar stamp-seals and shaft-hole axes point in the same direction. But he also draws attention to a first-rate piece of evidence, hitherto unrecognized, showing that people related to the Sialk Cemetery B folk were living in North Baluchistan, presumably about 1000 B.C. Cairns in a small cemetery at Moghul-ghundai dug by Stein produced a whole array of bronze objects—bells, rings, arrowheads, a bangle and a tripod jar—absolutely typical of the Sialk B tombs. To this may be added the arrowheads and greyware spouted pot fragments of similar affinities from Nad-i-Ali in Afghan Sistan, and, as I think we may now recognize, much of the pottery from the cemeteries of Zangian and Jiwanri in the Makran published by Stein (vessels comparable with the Sialk *cruches à bec aplati* for instance).

And finally, recent field-work in Baluchistan by Miss Beatrice de Cardi has produced even more convincing evidence of such connections. From a site at Londo, in Baghwana, north-west of Nal (from which Stein had picked up a few sherds) Miss de Cardi was able to obtain abundant material comparable in many ways to the Jiwanri style of painting, and two sherds of exceptional interest. One, with a design in black and white paint on a red background, has a pair of addorsed animals with long horns but otherwise equine features and bristly manes very similar in style to many of the Sialk B animals. The other (black-on-red) has part of a frieze of horses in exactly the manner of the animals (horses and others) on the famous long-spouted jugs from the Sialk Cemetery B: the Londo sherd is a provincial version of the fine Sialk pottery, but the stylistic affinities are quite clear.

It looks therefore as though contacts between India and the west were either continuous for nearly a millennium after 2000 B.C., or, more probably, that there were two main movements of peoples from Northern Persia towards India, one (Shahi-tump, Jhukar, etc.) fairly soon after 2000 B.C. (the Indian evidence implies that it cannot be earlier) and the other (Moghul-ghundai, Jiwanri, Londo) round about 1100 or later. In one or both of these the Aryans must be involved, and elusive though these people have been in the past, archaeologically considered, I feel that it will not be very long before more definite evidence of their migrations comes to light.

STUART PIGGOTT.

(NOTE: The Londo finds are referred to by the courtesy of Miss de Cardi and of the Director of the Institute of Archaeology, University of London, where the material is now housed).

GLASS THROUGH THE AGES. By E. BARRINGTON HAYNES. 240 pp., 64 plates in photogravure. Penguin Books, 1948. 2s.

Whoever attempts the story of glass in so short a compass must concentrate on a few of the highlights. Mr Haynes has chosen two. The first part of the book sketches the history of glass from its beginnings in Dynastic Egypt to the acme of the unmechanized industry in the 18th century and lays most emphasis on the essential unity of that development; he carries us rapidly back and forth between east and west at different eras and shows how there was never any appreciable dissociation of Levantine and western workers, however thin the connecting threads may have become in Dark Age times. In his second part he chooses a portion of the subject for exhaustive treatment



and provides a full classification of English 18th century glasses. Written mainly from the collectors' standpoint, Mr Haynes's book will nevertheless appeal equally to archaeologists and art-historians, and the copious illustrations, which stress the ordinary rather than the extraordinary, are alone worth more than the modest price of this Penguin book.

D. B. HARDEN.

FIVE THOUSAND YEARS OF PAKISTAN. By R. E. M. WHEELER. pp. 150, 21 plates and 19 figures. *Royal India and Pakistan Society, London.* 1950. 31s 6d.

We are told in the preface that the purpose of this book is to present a brief sketch of the imposing material heritage of Pakistan; it is however sub-titled 'an archaeological outline', and, in view of the fact that it is Prof. Mortimer Wheeler who is the author, anyone acquiring this book will, I am sure, expect it to be a handbook of the archaeology of East and West Pakistan.

I believe that ten additional pages of text, two more maps and about five extra plates would have made a considerable difference. This is open to the criticism that yet more pages, maps and plates would have made it better still; with this however I disagree. By these remarks I do not wish to convey that this is not a good book, but that with comparatively small additions it might have been a better one. Particularly welcome are the attempts to bring the past to life by narrative descriptions of the city of Mohenjodaro, the invasion of the Aryans and the arrival of Alexander at Taxila, all of which are archaeologically sound and help to make the dry bones live.

The first ten chapters contain good, clear, concise accounts of the cultures and industries of the Pakistan-Persian borderlands, of the Harappa civilization, the coming of the Aryans and the Buddhist period. The chapter on Hindu temples and fortifications is very welcome, as these particular examples are relatively little known, and so also is the plan of northern Kafir Kot. This fortress had not however the strategic value of the city of Akra, seven miles south of Bannu, which is not mentioned. Some reference might have been made to the fact that there are sites of the early historic period at all the points where rivers flow from the Waziristan gorges, and that Akra, commanding the Kurram and the Tank Zam, was the most important of them all.

The Islamic material is well handled, and a number of buildings which have hitherto been overshadowed by those of Delhi and Agra are brought into the prominence they deserve; that of East Pakistan however suffers from being intrinsically rather mediocre, and this part of the book is only relieved from dullness by occasional historical anecdotes. Prof. Wheeler very rightly stresses the various processes and techniques employed in the production of objects of everyday use, and concludes with a plea that the many humble links with the past, now either regarded as commonplace or disregarded altogether should be recorded and where possible preserved before they disappear and are forgotten.

One can only judge that Prof. Wheeler felt that it would not be right to use Sir Aurel Stein's unpublished material on Las Bela and Bahawalpur, but by its omission we are deprived both of part of the archaeological heritage of Pakistan and of the chance of some new light on the inter-relations of the Amri, Harappa and, if I may use my own designation, the Ravi cultures, all of which one gathers are represented in Bahawalpur. No mention is made of the microlithic finds in southern Sind and at Jamalgarhi, which help to link the early stone industries of the Rawalpindi and Campbellpur Districts, where a few micros have also been found, with the arrival of the peasant communities from Iran. The important iron age cairn burial people, whose distinctive pottery, along with their burial cairns and iron tools, is found from Hazar Mardi in the Rudbar tract of SE Iran to

Moghul Ghundai in the Zhob, should have been introduced as one of the few cultures in this area that can be plausibly attributed to the middle centuries of the 1st millennium B.C.

I feel that the rock engravings of Mandori and other Middle Indus sites and the rock paintings of Chargul should have received notice and the former linked with the little-known engravings, unfortunately unrecorded save by description, found by Air Surel Stein in Baluchistan. While dealing with the Buddhist period, though the importance of the Bala Hissar is again rightly stressed, Sar Dheri is not mentioned nor the terracotta figurines which are such a feature of the Yusufzai sites and of Taxila. Not enough credit is given to the Parthians for the spread of Hellenic ideas. Though it is true that they maintained a form of iron curtain, this was largely that they might retain their valuable position as middlemen on the land trade routes, and their influence must have been at least as much as that of the merchants of Alexandria, in that they set the fashion in the East even if the merchants produced the goods.

Some of the points mentioned and others not mentioned might well have occupied the ten additional pages. The two extra maps are needed to show those places in East and West Pakistan mentioned in the text, and a more ample and less select bibliography would have been of more value to the student than the list of Protected Monuments, which in any case will have little or no interest for the general reader.

All this does not detract from the fact that there is a very great deal of valuable information in this book most ably set out, and where material has been omitted it is I feel because our exact information about it is judged by the author to be too scanty. This may be right, but it deprives the picture of material, which, though it can only be sketched in lightly, does help to complete the design.

D. H. GORDON.

HERDSMEN AND HERMITS. By T. C. LETHBRIDGE, foreword by T. D. KENDRICK. Cambridge (Bowes and Bowes), 1950. 10s 6d.

The exchange of compliments between Mr Tom Kendrick and Mr Tom Lethbridge with which this book opens provides the most engaging of introductions. We soon after learn that the author is no academic archaeologist. He does not believe that humanity in the past, any more than in the present, can be studied from a library, or even from a motor road. He himself has farmed the lands and sailed the waters he is to talk of. He is in a very strong position.

He is a foe to jargon. Those persistent potters the 'beaker folk' are his *bêtes noires*, especially if qualified A, B or C. He shows from his Greenland excavations to what a level of unmeaningness a tribe can be reduced if judged by its one surviving artifact to the exclusion of its multifarious equipment in more perishable materials. He eschews 'impacts', 'impulses', 'influences': all those abstract terms which may mean anything or nothing in terms of the human beings which are the subject of archaeology, but so readily become subjects of discussion in themselves. For all this we can bless Mr Lethbridge, remembering that the *Decline and Fall* was due to his university not giving Gibbon a research scholarship and a lectureship to teach Greats students, but leaving him to make love to the future wife of Necker, to practise soldiering in Hampshire and to sit as a Trade Commissioner in Whitehall.

But Gibbon was at the same time a patient master of his facts, and here Mr Lethbridge is not always so strong. Accordingly his book falls into two parts of unequal value, the one dealing with those matters he knows from his own experiences, the other with those impressions he has gained from reading the works of others. It is good indeed to have these impressions, and the writers criticized will not mind that they are sometimes

## REVIEWS

reproved for not saying what in fact they did say—Prof. Childe, for example, for *not* saying the Skara Brae community was a single great family. But we do not achieve 'a clear picture of the real Bronze Age in Scotland' by alleging that there are 'no traces of settlements with characteristic pottery and other relics', or of the cult of the dead among users of 'grooved ware' by ignoring the occurrence of their pottery in unurned cremation enclosures.

Let us turn to Mr Lethbridge's direct experiences. There is nothing better than his climb up the fowling cliffs of the Westman Island southwest of Iceland and his discovery, carved on the rock face, of a cross of a type known in the Scottish Islands. Whether this proves that the Westmen Islands were occupied by Celtic hermits before the Norse settlement, or, as the story in the *Landnamabok* rather suggests, by a Celtic community of escaped slaves, or whether it proves neither, the story has the immediacy of experience and is to be welcomed as such.

Valuable also is the observation that some Portuguese boats preserve the hull shape of Mediterranean 2nd millennium boats known to us from Egyptian grave models. Mr Lethbridge would derive the broken sheer of some Irish currachs from this type, and he develops an enthusiasm for the deep-sea capabilities of the currach which it is hard to share. One would feel happier about the 2nd millennium settlements in Western Scotland if wooden boats of the Egyptian type (five-part canoes) had been available, as tomb carvings extending at least as far west as Sardinia suggest they were. On the currachs we can more safely listen to the sarcastic comment in a medieval Gaelic poem on a ship 'without oak timber' as 'all one ship of leather; she is not a ship complete for sea-going'.

In more scholarly vein is the suggested development of pins in Western Britain. In the scarcity of pottery the pin may prove a clue to Dark Age chronology and the discussion is valuable; a fuller account is promised in the required detail and we hope we shall not wait for this in vain.

Of such varied materials is the book composed. Not all will be equally valuable to remember; but this is not a book of reference, and we may be grateful for those lively and first-hand impressions which we shall not want to forget. L.S.

WHITHORN VOLUME. *Ed. R. C. REID. Transactions of the Dumfriesshire and Galloway Natural History and Antiquarian Society, Third Series, Vol. XXVII, Dumfries 1950, 245 pp. plus illustrations. No price.*

The society and its energetic editor deserve praise for the plan, conceived and executed, of devoting one annual volume almost entirely to articles on Ninian and Whithorn. Mr Raleigh Radford's preliminary report on the 1949 excavations at Whithorn raises the volume from local to national importance. A collection of essays on St. Ninian is bound to reveal inevitable clashes of opinion, and some contributions are highly speculative, if not imaginative. In two of the shorter articles photography and reproduction leave much to be desired. But the idea of a 'Whithorn Volume' is excellent; it deserves to be imitated and developed.

F. T. WAINWRIGHT.



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A QUARTERLY REVIEW OF ARCHÆOLOGY



*Edited by*  
*O. G. S. Crawford, C.B.E., F.B.A.*

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